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THE

PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND
THEORETICAL PHYSICS

CONDUCTED BY

EDWARD L. NICHOLS, ERNEST MERRITT,
AND FREDERICK BEDELL

VOL. IX

NEW YORK
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PUBLISHED FOR CORNELL UNIVERSITY
THE MACMILLAN COMPANY
NEW YORK LONDON
BERLIN : MAYER AND MUELLER
1899

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CONTENTS OF VOLUME IX

XLV. JULY, 1899

PAGE

Frontispiece

An Interferometer Study of Radiation in a Magnetic Field. I. JOHN C. SHEDD . . . 1

The Electrical Resistance of Lead Amalgams at Low Temperatures. GEORGE W. GRESSMAN . . . 20

The Wehnelt Electrolytic Break. W. J. HUMPHREYS . . . 30

The Hydrolysis of Stannic Chloride. WILLIAM FOSTER, JR. . . . 31

Notes: Gustav Wiedemann and Wilhelm Gottlieb Hankel . . . 57

New Books: *Silas W. Holman:* Matter, Energy, Force and Work; Harper's Scientific Memoirs: Vol. III., Roentgen Rays; Vol. IV., Modern Theory of Solution 59

XLVI. AUGUST, 1899

The Specific Heat of Solutions which are not Electrolytes. WILLIAM FRANCIS MAGIE . . . 65

An Interferometer Study of Radiations in a Magnetic Field. II. JOHN C. SHEDD . . . 86

The Effect of Magnetization upon the Elasticity of Rods. J. S. STEVENS and H. G. DORSEY . . . 116

On Freezing and Boiling Water Simultaneously. R. W. QUICK . . . 121

New Books: *P. Q. Tait:* Scientific Papers; *A. Bourgougnon:* Physical Problems and their Solutions; *Ostwald:* Klassiker der Exakten Wissenschafter, Nos. 61, 63, 72, 75, 76, 79, 80, 81, 82, 83 . . . 123

XLVII. SEPTEMBER, 1899.

A New Respiration Calorimeter and Experiments on the Conservation of Energy in the Human Body. I. W. O. ATWATER and E. B. ROSA . . . 129 ✓

A Method for the Study of Phosphorescent Sulphides. FRED. E. KESTER . . 164

The Photometric Study of Mixtures of Acetylene and Hydrogen Burned in Air. LEON W. HARTMAN . . . 176

A Photographic Study of Electrolytic Cells. ROLLA R. RAMSEY . . . 189

New Books: *Tarleton:* An Introduction to the Mathematical Theory of Attraction; *Todd:* Stars and Telescopes . . . 191

XLVIII. OCTOBER, 1899.

On the Magnetic Shielding Effect of Tri-lamellar Spherical and Cylindrical Shells. A. P. WILLS . . . 193

A New Respiration Calorimeter and Experiments on the Conservation of Energy in the Human Body. II. W. O. ATWATER and E. B. ROSA . . . 214

New Books: *Hyndman:* Radiation, An Elementary Treatise on Electromagnetic Radiation and on Röntgen and Cathode Rays . . . 252

Notes: American Association for the Advancement of Science; International Congress of Physics . . . 254

XLIX. NOVEMBER–DECEMBER, 1899.

PAGE

Frontispiece

On the Freezing Points of Aqueous Solutions of Non-electrolytes. E. H. LOOMIS 257

An Absolute Determination of the E. M. F. of the Clark Cell. HENRY S. CARHART and KARL E. GUTHE 288

The Resistance Offered by Iron Wires to Alternating Currents. ERNEST MERRITT 294

On a Theorem of Clebsch's. PAUL SAUREL 300

An Investigation of the Magnetic Qualities of Building Brick. O. A. GAGE and H. E. LAWRENCE 304

Note: Robert Wilhelm Bunsen 310

New Books: *Hastings and Beach:* A Text-Book of General Physics; *William Watson:* Practical Physics: *Harper's Scientific Memoirs:* The Laws of Gases; The Second Law of Thermodynamics; The Fundamental Laws of Electrolytic Conduction 313

Index 318

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Volume IX. *July, 1899.* *Number 1.*

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LENOX AND

THE PHYSICAL REVIEW.

AN INTERFEROMETER STUDY OF RADIATIONS IN A MAGNETIC FIELD. I.

✓

JOHN C SHEDD.

PART ONE. HISTORICAL SURVEY.

IN 1845 Faraday discovered the rotation of the plane of polarization due to a magnetic field of force. This, perhaps, suggested the further experiment as to the effect of a magnetic field upon a source of radiation placed within it.[1]

In the light of what is now known Faraday's repeated failure to obtain positive results from this last experiment may be ascribed to the low dispersive power possessed by his apparatus, and to the comparatively low temperature of the gas flame used as a source of radiation.

In 1865 Maxwell propounded the electromagnetic theory of light, which not only correlated all hitherto observed phenomena, but also furnished a scientific basis for future research.

In 1875 Professor Tait[2] presented a paper "On a Possible Influence of Magnetism on the Absorption of Light" which, while not realized by him experimentally is of interest in this connection. He says in part:

"The explanation of Faraday's rotation of the plane of polarization of light by a transparent diamagnetic requires, as shown by

---

[1] Appendix I.

[2] Proc. Roy. Soc. Edinburgh. Sessions 1875–76, p. 168.

Thomson,[1] molecular rotation of the luminiferous medium. The plane polarized ray is broken up, while in the medium, into its circularly polarized components, one of which rotates with the ether so as to have its period accelerated, the other against it in a retarded period. Now, suppose the medium to absorb one definite wave-length only, then—if the absorption is not interfered with by the magnetic action—the portion absorbed in one ray will be of a shorter, in the other, of a longer period than if there had been no magnetic force; and thus, what was originally a single dark absorption line might become a double line, the components being less dark than the single one."

The line of reasoning here presented, if applied to a source of radiation (instead of absorption) placed in a magnetic field would give an analogous solution of two bright lines of less intensity than the original line.

The first experimental results in this field of work were obtained by M. Fievez[2] in 1885–86. His observations consisted in a broadening of the spectral line, and increased brilliancy of illumination. No observations are recorded as to the state of polarization.

The phenomena of reversal and increased brightness of the lines, while undoubtedly precipitated by the action of the magnetic field, were probably due, primarily, to changes of temperature and density. The broadening of the line, however, was undoubtedly a genuine magnetic effect, and as Preston[3] remarks, had Fievez been acquainted with the theory of the subject the whole question would have been settled in 1885.

Fievez does not seem to have been familiar with what Faraday had done as regards the state of polarization, nor to have taken any special precautions against spontaneous reversals; yet, while his work may, in the light of recent investigations, appear meager, it deserves an important place in the history of the subject, and must sooner or later have led to the very results reached by Zeeman.

A paper entitled "Causes of Double Lines and Close Satellities in the Spectra of Gases," by G. J. Stoney,[1] though not considering the special case of magnetic forces, has, nevertheless, a theoretical bearing upon the subject.

---

[1] Reprint Thomson's Papers on Electrostatics and Magnetism. Second Edition, p. 423, Footnote.

[2] Appendix II.

[3] Phil. Mag. (5), 45, 1898, p. 338.

Of still greater importance are the writings of Lorentz,[2] published in 1892 and 1895, inasmuch as they were a guide to Zeeman in his experiments.

In March, 1897, Dr. P. Zeeman[3] communicated to the Philosophical Magazine the results of a research that has proved wonderfully fruitful in his own hands and also in the hands of others. Zeeman was familiar with Faraday's experiments, but did not know of Fievez's work. He was influenced, as a matter of course, by Maxwell's electromagnetic theory of light, and in particular was guided by Lorentz's exposition of it. His first experiments were identical with those of Faraday, excepting that he had a Rowland grating of 14,000 lines to the inch, and hence had a much greater dispersion. With this arrangement of the apparatus, a broadening of the spectral line was observed, similar to that seen by Fievez. Guided, however, by Lorentz's theory, Zeeman tested the state of polarization of the broadened line, and found that, when viewed parallel to the lines of force, the edges of the line were circularly polarized as predicted by Lorentz's theory.

Lorentz's Theory.—In this theory it is assumed that light vibrations are the vibrations of electrically charged ions of definite mass. Thus suppose such an ion having a charge e and mass m, to vibrate with simple harmonic motion about its center of equilibrium. Such an ion moving in a magnetic field would then experience mechanical forces, which would cause a change of period of vibration. The amount of this change of period would depend upon the ratio e/m, and the measurement of the change of period would give a knowledge of this ratio.

The equations giving the modified period are derived as follows.[4]

In Fig. 1 let the origin be at o and the axis of Z be parallel to

---

[1] Sci. Trans. Roy. Dublin Soc., Vol. IV., p. 563, 1891.

[2] Lorentz, "La Theorie electromagnetique de Maxwell." Leyden, 1892. "Versuch einer Theorie der electrichen und optischen Erscherungen in bewegten Korpern." Leyden, 1895.

[3] Phil. Mag. (5), 43, p. 226. Same art. Astro. Phys. Jr., 5, p. 332, 1897.

[4] See Zeeman, Phil. Mag. (5), 43, p. 226.

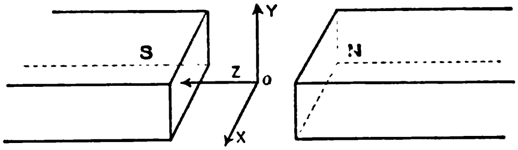

Fig. 1.

the lines of force, the axes of X and Y being perpendicular to this direction. Then if H be the strength of field, the equations of motion relative to the X and Y axes are

$$\left.\begin{aligned} m\frac{d^2x}{dt^2} &= -K^2x + eH\frac{dy}{dt} \\ m\frac{d^2y}{dt^2} &= -K^2y - eH\frac{dx}{dt} \end{aligned}\right\} \qquad (1)$$

The term K is the coefficient of elasticity of the ion, the second term gives the mechanical force due to the magnetic field.

The solution of these equations yields for the period of vibration the following values:

$$\text{If} \quad H = 0, \quad T = \frac{2\pi\sqrt{m}}{K} \qquad (2)$$

If H be not zero, and we regard all forces in the XY plane as symmetrcial with respect to the axis of Z, then

$$T = \frac{2\pi\sqrt{m}}{K}\left(1 \pm \frac{eH}{2K\sqrt{m}}\right). \qquad (3)$$

The meaning of equations (2) and (3) is as follows:

If we analyze along the three coördinate axes, the motion of the moving ion, which by virtue of this vibration is emitting light, we shall have three component vibrations of equal period. We may next compound the two components which lie in the XY plane into circular motion, and since we do not know the sense of rotation, we may conceive it to be in both directions simultaneously.[1]

If the source of light be viewed in a direction parallel to the magnetic field, the component whose motion is parallel to the axes of Z is not effective, and the ray is, therefore, composed of circularly polarized components, whose periods of rotation are the same, and for zero magnetic field have the value given by equation (2). If the magnetic field be equal to H, then the periods become changed, and are given by equation (3). The periods being changed by equal and opposite amounts, the original spectral line becomes two lines of equal intensity, symmetrically situated with respect to the original unmagnetized line; the components being circularly polarized in opposite senses.

---

[1] The sense of rotation will depend upon the sign of the charge e upon the iron.

Viewed in a direction normal to the magnetic field, the component parallel to the axis of Z, and, therefore, parallel to the axis of the magnetic whirl, is not affected by it, is unaltered in period and appears as a plane polarized ray. The two circular components, being now viewed parallel to the $X\,Y$ plane, also appear as plane polarized rays, the plane of polarization being normal to that of the Z component.

Thus viewed, the spectral line is broken up into *three* component lines, and if the magnetic field is of sufficient intensity there will be seen *three* distinct lines, the two outlying ones having their vibrations in a plane normal to the magnetic field, and the central one in a plane parallel to it. If the magnetic field is not powerful enough to separate the three components they will overlap, and the line appears merely broadened. The components may, however, be isolated by means of a nicol prism.

In his earlier work[1] Zeeman obtained only a broadened line, but by means of the nicol he was able to test the state of polarization; and, later,[2] he succeeded in obtaining both the doublet parallel to the magnetic field, and the triplet normal to the field, as predicted by Lorentz's theory.

Methods.—In the further progress of the work two radically different methods have been developed.

The first is that followed by Zeeman and many other investigators. It consists in photographing the spectral lines, and in measuring the separation of the "magnetized" components by means of the micrometer dividing engine. The chief merit of this method is that a permanent record is secured in the photograph. It is also

[1] Phil. Mag. (5), 43, p. 226.

[2] Phil. Mag. (5), 44, pp. 55 and 255; 45, p. 197.

an advantage that the phenomenon is observed directly. The limitations of the method are: *First*, the fact that the quantity to be measured is minute, and the micrometer method of direct measurement is of necessity limited in range; *Second*, it frequently happens that the components, whose distances apart are to be measured, are so nebulous as to make it exceedingly difficult to make exact micrometer settings. *Third*, the time of exposure necessary is sometimes so long as to make the method prohibitory.

The second method is due to Professor A. A. Michelson, and may be called the *Interferometer Method*. It has been used successfully with magnetic fields far too weak to give any sensible effect by the direct method, and has been shown to have a delicacy and sensitiveness far in excess of any photograph. The method, as used by Professor Michelson, consists in obtaining the visibility curves of the spectral line, both when unmagnetized, and also with fields of different intensity. These visibility curves are then analyzed,[1] and the distribution of light at the source of illumination obtained. This gives directly the various "magnetic" components of the line: By means of a nicol prism the two planes of polarization may be separately examined.

The advantages of this method are—briefly—*First*, the visibility curve enables the separation of lines not hitherto resolved by any other method. *Second*, the eye is the instrument of investigation, and hence there is no need of long exposure as in the case of taking photographs. *Third*, any change of polarization—or other effect—taking place during the period of observation may be detected, while the photographic process is necessarily an integrating method.

The disadvantages are: *First*, the method is an indirect one, *i. e.*, the observations are made not on the lines themselves but on interference fringes produced by them. *Second*, the accurate estimate of a visibility curve is by no means an easy matter, and the rare success attained by Professor Michelson has been equalled by no one else, and can only be approached by practice and great patience. *Third*, the record of the instrument is not automatic, as in the photograph, and is subject to the personal error of the observer. *Fourth*, the reflection from the half silvered surface of the interferometer affects the two beams polarized in perpendicular planes, to a different degree, so that when both beams are simultaneously observed they have not their normal ratio of brightness, with the result that the fringes are correspondingly deceptive.

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[1] Phil. Mag. (5), 34, p. 280, 1892. Astro. Phys. Jr., 7, p. 129, 1898.

In the hands of an experienced observer the interferometer is undoubtedly the most powerful instrument of attack that is available at present, unless, indeed, Professor Michelson has presented in the Eschelon Plate Spectroscope, an instrument of equally great value. Under circumstances less favored than those enjoyed by Professor Michelson, it is difficult to see how the Interferometer Method as he uses it, can be successfully used. There are, however, modifications rendering this instrument more available, which have been used by the present writer.

Experimental Results.—The agreement between theory and experiment presented by Zeeman's early experiments was truly remarkable, and the apparent simplicity of the phenomenon seemed equally worthy of notice. This apparent simplicity, however, was soon found not to be true of all spectral lines, and more complex forms were found. Exceptions were also found to the state of polarization as first described by Zeeman.

The first observer to note a departure from the normal form was M. Cornu.[1] His apparatus was similar to that of Zeeman excepting that he used a double image prism, and was thus able to observe both planes of polarization simultaneously. For a magnetic field strength of 13,000 C. G. S. units he observed that the sodium line, D_1 when viewed normal to the magnetic field was a quadruple, the inner components being polarized perpendicular to the magnetic field and the outer ones parallel to this direction. The line, D_2 he found to be a hazy triple with each member perhaps doubled.

M. Cornu was soon followed by others. Preston[2] succeeded in photographing as many as five different types, and Michelson[3] with the interferometer showed three well marked groups of lines.

As regards polarization Becquerel and Deslandres[4] have found that one of the iron lines when viewed perpendicularly to the magnetic field becomes a triple, in which the usual state of polarization is reversed. The same phenomenon has also been observed at the Johns Hopkins[1] University.

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[1] Astro. Phys. Jr., 6, p. 378, 1897; 7, p. 163, 1898.

[2] Phil. Mag. (5), 45, p. 330, 1898; 47, p. 165, 1899.

[3] Phil. Mag. (5), 44, p. 109, 1897, same Art. Astro. Phys Jr., 6, p. 48, 1897; Phil. Mag. (5), 45, p. 348, 1898, same Art. Astro. Phys. Jr., 7, p. 131, 1898.

[4] C. R., April 4, 1898.

The classification of lines as given by Preston is shown in Fig. 2, who thus describes them.

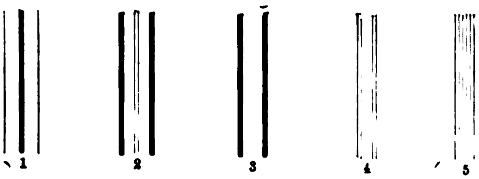

Fig. 2.

"In 1 we have the normal triplet. In 2 we have the weak middled 'quartet' in which nearly all the light is concentrated in the two side lines. Next we have in 3 the doublet in which the central line has completely disappeared. Next in 4 we have the double doublet, or two pairs of fine lines, and finally in 5 the sextet or three pairs of equally spaced sharp lines."

Preston[2] cites the following as examples of the above types: Type 1, Cd, 4678; Mg, 5167; Zn, 4680 and the vast majority of other lines. Type 2, Mg, 5183; Cd, 5086; Zn, 4810. Type 4, Mg, 5173; Cd, 4800; Zn, 4722. In his latest work[3] Preston seems to restrict his classification to three types, viz.: "Diffuse triplets," "quartets" and "pure triplets." It may be that types 3 and 4 are modified forms of the same type, as also 2 and 5. This would leave but three types.

The types found by Michelson[4] are shown in Fig. 3. The upper curves are taken by the interferometer and the lower by the Echelon plate spectroscope. The spectral lines are viewed normal to the magnetic field in both figures 2 and 3.

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[1] Astro. Phys. Jr., 8, p. 48, 1898.

[2] Phil. Mag. (5), 45, p. 330.

[3] Phil. Mag. (5), 47, p. 178. See also Nat., Vol. 59, p. 226, where seven types are given.

[4] Astro. Phys. Jr., 7, p. 136, 1898. Nat., March 9, 1899.

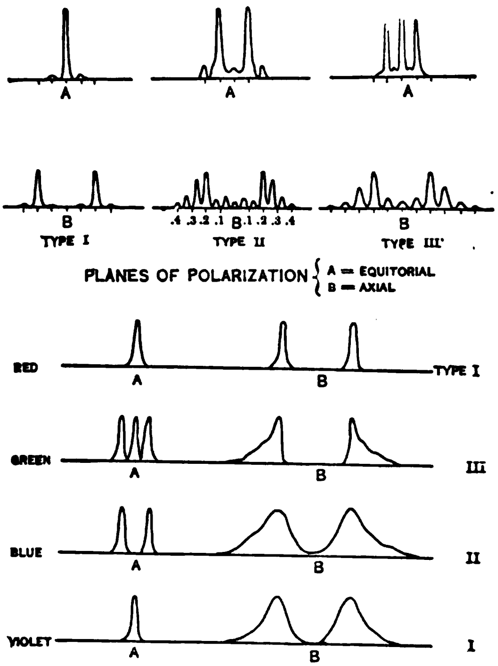

Fig. 3.

The following are some examples:

Type I.		Type II.		Type III.	
Hg	yellow line.	Hg	violet line.	Hg	green line.
Cd	red line.	Cd	blue line.	Cd	green line.
Zn	red line.	Zn	blue line.	Mg	green line (5183).
Au	green line.	Na	yellow line.		
Ag	yellow line.	Au	yellow line.		
Ag	green line.				

Michelson adds a fourth type in which a broad or complex line is simplified or narrowed in the magnetic field. Examples of this are Cu yellow line and Mn green line (5340). This effect is true of the central member of the triplet in the case of these two lines.

If now a comparison be made between such lines as have been observed by several persons the following rather meager data are found.

TABLE I.

Lines.	Type.	Field Strength.	Observer.
Zn. 4810.	Diffuse triplets.	———	Preston.
" "	Type II	10,000.	Michelson.
Cd. 5086.	Diffuse triplets.	———	Preston
" "	Type III.	10,000.	Michelson.
" 4800.	Quartets.	———	Preston.
" "	Type II.	10,000.	Michelson.
Na. D_1	Type II.	10,000.	Michelson.
" "	Quartet.	13,000.	Cornu.
" D_2	Type II.	10,000.	Michelson.
" "	Sextet.	13,000.	Cornu.

The results of such a comparison are of little value, unless the conditions as regards the source of radiation and the magnetic field strength are known to be the same. Two things are, however, apparent.

I. The phenomenon is by no means as simple as was at first supposed by Zeeman.

II. The superiority of the Interferometer Method as regards resolving power is shown.

Modified Theories.—Having found that the extremely simple deductions from Lorentz's theory, as made by Zeeman, do not comprehend the observed phenomena, it becomes necessary, either to present a new hypothesis or suitably to modify the old one. This modification has been made by Lorentz,[1] Larmor[2] and others, while Preston[3] has pointed out that the paper of Dr. Stoney,[4] already cited, anticipates the desired theory.

Dr. Stoney considers the effect of perturbing forces upon an ion, moving in an elliptical orbit under the action of a central force, which is proportional to the distance. If the perturbing force be such as to cause the orbit to rotate in its own plane, then the spectral line becomes a doublet. Thus if the ion be moving with an angular velocity Ω then $\Omega = 2\pi N$ where N is the frequency of the rotation. If now an angular velocity of ω be impressed upon the system as a whole, and in the same plane as Ω, then the resultant angular velocity is the algebraic sum of the two. The resultant frequency will also be the algebraic sum; and since Ω is to be regarded in both senses, the resultant motion will have a double frequency of $N + n$ and $N - n$.

---

[1] Proc. Roy. Acad. Sci. Amsterdam, June 25, 1898. Also Astro. Phys. Jr., 9, p. 37. Wied. Ann., Bd. LXIII., p. 278, 1897.

[2] Phil. Mag. (5), 44, p. 503.

[3] Phil. Mag. (5), 47, p. 171.

[4] Trans. Roy. Soc. Dublin, Vol. IV., p. 563. See also Preston, Phil. Mag. (5), 47, p. 171.

Viewed dynamically the equations show what forces are necessary to produce the above changes. If the ion rotate with an angular velocity Ω, and the orbit itself rotate about an axis passing through the center of force, and having a direction (lmn) with an angular velocity ω, then the component velocities referred to the rotating orbit are

$$\left.\begin{aligned} u &= \frac{dx}{dt} = -\omega ny + \omega mz \\ v &= \frac{dy}{dt} = -\omega lz + \omega nx \\ w &= \frac{dz}{dt} = -\omega mx + \omega ly \end{aligned}\right\} \tag{4}$$

The component accelerations are

$$\left.\begin{aligned} \frac{du}{dt} &= -\omega nv + \omega mw \\ \frac{dv}{dt} &= -\omega lw + \omega nu \\ \frac{dw}{dt} &= -\omega mu + \omega lv \end{aligned}\right\}. \tag{5}$$

Expanding equation (5) from equation (4) the acceleration along the axis of X is

$$\frac{du}{dt} = \frac{d^2x}{dt^2} = -\omega n\frac{dy}{dt} + \omega m\frac{dz}{dt} - \omega n\left(\frac{dy}{dt} - \omega lz + \omega nx\right) + \omega m\left(\frac{dz}{dt} - \omega mx + \omega ly\right)$$

which, by adding the term $\omega l(\omega lx - \omega lx)$ and remembering that $l^2 + m^2 + n^2 = 1$, reduces to

$$\frac{d^2x}{dt^2} = -2\omega\left(n\frac{dy}{dt} - m\frac{dz}{dt}\right) - \omega^2 x + \omega^2 l(lx + my + nz).$$

Two similar expressions give the acceleration along the Y and Z axes.

The total acceleration experienced by the ion is then

$$\begin{aligned} X &= -\Omega^2 x - \frac{d^2x}{dt^2} = -\Omega^2 x + 2\omega\left(n\frac{dy}{dt} - m\frac{dz}{dt}\right) \\ &\quad + \omega^2 x + \omega^2 l(lx + my + nz) \\ Y &= -\Omega^2 y - \frac{d^2y}{dt^2} = \text{etc.} \\ Z &= -\Omega^2 z - \frac{d^2z}{dt^2} = \text{etc.} \end{aligned} \qquad (6)$$

In the case of a magnetic field of force, if the axes of Z be taken parallel to the magnetic field then (l, m, n) become equal to $(0, 0, 1)$ and equations (6) reduce to

$$\left.\begin{aligned} X &= -\Omega^2 x + \omega^2 x + 2\omega\frac{dy}{dt} \\ Y &= -\Omega^2 y + \omega^2 y - 2\omega\frac{dx}{dt} \\ Z &= -\Omega^2 z \end{aligned}\right\}. \qquad (7)$$

Now the central force producing the original rotation is $\Omega^2 x$. The perturbing forces are then represented by the terms $\omega^2 x + 2\omega\frac{dy}{dt}$ and $\omega^2 y - 2\,\omega\frac{dx}{dt}$. Examining these it is seen that $2\omega\frac{dy}{dt}$ and $-2\omega\frac{dx}{dt}$ are the X and Y components of a force $2\omega v$ acting perpendicularly to v, the linear velocity of the ion. If then a charged ion move in a magnetic field with a velocity v, $2\omega v$ is the force it would experience due to the magnetic field. The terms $\omega^2 x$ and $\omega^2 y$ represent centrifugal forces due to the impressed velocity ω, and in the first approximation may be neglected. Finally, if $K = 2\omega$, the above equations become identical with those of Lorentz.

Equations (6) and (7) are sufficiently general to cover all hitherto observed phenomena. Thus to explain the case where the central line of the triplet (line 2, Fig. 2) is doubled, it is only necessary to write the equation for Z in the form $Z = A \sin \Omega t$ where A is a periodic function of t of the form $A = a \sin nt$. Substituting this in the equation $\frac{d^2z}{dt^2} = -\Omega^2 z$ and integrating we get

$$Z = a \sin nt \sin \Omega t = a/2 \left[\cos(\Omega - n)t - \cos(\Omega + n)t\right],$$

which represents two vibrations of frequency

$$(\Omega \pm n)/2\pi. \tag{8}$$

The case of reversed polarization may be covered by supposing the value of n to be such as to separate the components sufficiently to place them outside the other lines of the triplet. In a similar manner the doubling of the outer members of the triplet may be accounted for which would also cover the case of a quartet when viewed parallel to the magnetic field. In this way all the various cases of multiple lines are satisfactorily explained.

Reverting to equations (2) and (3) we see that the change of period of the ion is expressed by the equation,

$$T - T' = \frac{2\pi\sqrt{m}}{K} \cdot \frac{eH}{2K\sqrt{m}}.$$

The proportional change will then be

$$\frac{T - T'}{T} = \frac{eH}{2K\sqrt{m}} = \frac{e}{m}\frac{HT}{4\pi}$$

since $$K = \frac{2\pi\sqrt{m}}{T}.$$

Finally, since $T = \frac{\lambda}{v}$ and $T' = \frac{\lambda'}{v}$ where v = velocity of light, this

becomes $$\frac{\lambda - \lambda'}{\lambda^2} = \frac{e}{m}\frac{H}{4\pi v}. \tag{9}$$

Where λ = wave-length of the spectral line for zero magnetic field.
λ' = wave-length of the spectral line with magnetic field.
v = velocity of light, 300,000,000 cm.
H = intensity of magnetic field in C. G. S. units.

Equation (9) may be written

$$\frac{e}{m} = \frac{\lambda - \lambda'}{\lambda^2} \frac{4\pi v}{H} \qquad (10)$$

and it is seen that a measurement of the change of wave-length enables us to determine the ratio of the ionic *charge* to the ionic *mass*.

Zeeman[1] finds for this ratio, in the case of the blue line of Cd ($\lambda = 4800$) 24×10^6, while Preston[2] has found that this value must be determined separately for each line, but that a possible classification of lines may be made similar to the chemical classification of Keyser and Runge.[3]

Another interesting and significant observation is that of Ames[4] et. alt. who find that in the case of some iron lines, there appears to be no magnetic effect, while those lines which show the greatest magnetic shift are the ones which show the greatest pressure shift, and those which show but little magnetic shift are the ones of little pressure[5] shift.

The present status of the subject may be summarized as follows:

1. In general, spectral lines are influenced by the magnetic field when the radiations emanate from a source of light in the magnetic field.[6] The magnetized system of lines is symmetrical with respect to the original unmagnetized line. New spectral lines may be produced by the magnetic field.[7]

2. Viewed parallel to the magnetic field the spectral line is, in general, doubled, but it may become a single or multiple line. In the case of a single line there is no polarization;[8] in all other cases the components are circularly polarized; the shorter wave-length in the direction of the magnetizing current, the longer wave-length in the opposite sense.

3. Viewed in a direction perpendicular to the magnetic field the

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[1] Acad. Amsterdam, 1897–1898, p. 260.

[2] Phil. Mag. (5), 45, p. 337.

[3] Wied. Ann., Bd. XLIII., p. 394, 1891.

[4] Astro. Phys. Jr., 8, p. 50, 1898.

[5] Astro. Phys. Jr., 6, p. 169, 1897.

[6] Astro. Phys. Jr., 8, p. 49.

[7] Astro. Phys. Jr., 9, p. 47.

[8] This would be true of the lines seen by Becquerel and Ames, showing reversed polarization, when viewed parallel to the magnetic field.

outer components are plane polarized so that their vibrations are perpendicular[1] to the magnetic field; the inner components having their vibrations parallel to the magnetic field.

4. The general form of the line viewed perpendicularly to the magnetic field is a triplet, but it is sometimes of complex structure.

5. The amount of magnetic action, measured in change of period of the spectral line, is not a simple function of the wave-length; nor is it a constant for all wave-lengths; nor a constant for all lines of a given substance; nor is it a simple function of wave-length for the lines of a given substance.

6. The magnetic action is proportional to the field strength (being limited, however, by temperature and pressure); and there appears to be a possible classification following the chemical classification of Mendelejeff and of Kayser and Runge.

PART TWO. EXPERIMENTAL WORK.

Introductory Note.—From the foregoing survey of the subject we are led to believe that a comprehensive study of the problem consists essentially of two parts:

1. A qualitative analysis of as many spectral lines, emanating from as many different substances as possible, with a classification according to the type of line produced.

2. Quantitative measurements of the change of wave-length and of the ratio e/m, and a classification based upon these measurements.

All spectral lines belonging to the same group in both classifications may then be regarded as possessing related properties, and the ratio e/m as determined from such a group of lines should have the same value.

Such a series of observations would as Preston remarks "afford a valuable means of inquiry into the so far hidden nature * * * of the radiation from a luminous body, and also give us some clearer insight into the structure of matter itself."

Outline of Work.—In the following research the complete study of the subject was not attempted, this being manifestly too great a task for the limited time available. The preliminary ground has, however, been cleared and a beginning made.

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[1] Exception, see Astro. Phys. Jr., 8, p. 50, 1898.

The work was subdivided as follows:

Section I. A preliminary survey of the field with a view of determining the conditions limiting the observation of the magnetic phenomenon.

Section II. A comparison of the ease of manipulation and range of the two methods outlined above.

Section III. To ascertain whether the magnetic effect is radically different at different temperatures.

Section IV. To measure the magnetic shift of as many lines as the time available would permit; studying, also, the state of polarization of the components.

SECTION I. A PRELIMINARY SURVEY TO DETERMINE THE CONDITIONS LIMITING THE OBSERVATION OF THE MAGNETIC PHENOMENA.

APPARATUS. I. *Magnet.*—This was of the usual upright type; the base, cores, coils, pole heads, and cores to pole heads are all separable; the base and cores to the pole heads are of mild steel, the rest of the magnetic circuit being made of that form of cast iron known as *Mitis Metal.* An elevation of the magnet is shown in Fig. 4, drawn to ⅛ scale.

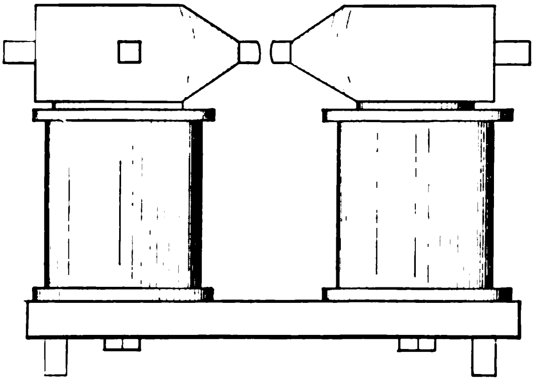

Fig. 4.

The cores to the pole heads are one inch in diameter, and it soon became apparent that they, together with the mass of iron behind them, tended to lower the temperature of the flame. A second pair of cores was made of the form shown in Fig. 5. This form also concentrated the magnetic field, thereby increasing its strength. A third core was prepared similar to those shown in Fig. 4, with a ¼-inch hole throughout its length; this core was for use in viewing the flame parallel to the magnetic field.

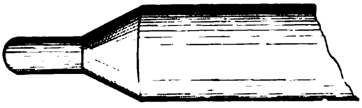

Fig. 5.

II. *The Flame.*—The flame of a small Bunsen burner was first used but was found to be too large. A small blast lamp was then made of glass, and a foot bellows used. This gave a small conical jet of flame about 3 inches high when the blast was inactive, and about ¾ inch high with the blast. This form of lamp, shown in Fig. 6, proved highly satisfactory.[1]

To color the flame a strip of asbestos wick supported by platinum wire was first used. A bead of fused sodium carbonate was also used, and, finally, a rod of sodium glass was adopted as needing the least attention. When a very bright flame is used recourse to the fused bead may be had, but such a flame generally gives rise to spontaneous reversals. In the latter experiments the glass lamp was set aside for the burner of an oxy-hydrogen lamp, and the foot-blast was replaced by an oxygen tank.

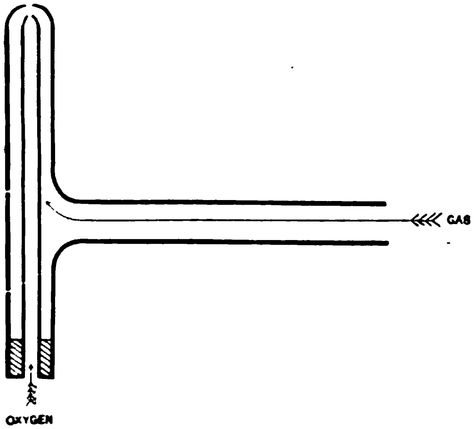

Fig. 6.

III. *Dispersion Apparatus.*—The first trials were made with a plane grating spectroscope; the higher spectra were especially dim. No success was attained with this apparatus. Next a Rowland concave grating of 14,436 lines to the inch and five feet focal distance was mounted at one end of the table, the magnet being at the other end. The spectrum was viewed with a telescope. It was with this mounting and with an oxygen gas flame that the first results confirmatory of Zeeman's work were obtained.

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[1] In time the glass about the opening cracks away but the whole lamp is easy of construction.

The arrangement of apparatus is shown in Fig. 7. The lines D_1 D_2 were very sharp and bright. When the current was turned on each line grew broad and after a few seconds became distinctly double, a sharp dark line separating the components throughout their whole length. The lines were visibly brightened, as was also the whole flame.

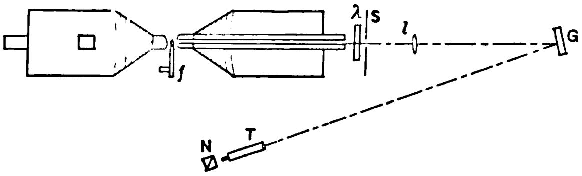

Fig. 7.

When the current was interrupted the lines appeared to collapse, and after a second or so would again become sharp. Frequently the doubling of the lines would persist for a few seconds after the current was broken and the dark dividing line could be seen, though narrower than with the full field.

For the doubling a field strength of about 18,000 C. G. S. units was used, with weaker fields only a broadening of the line could be observed.

Polarization.—With zero field the light was found to be slightly polarized by reflection from the grating. The polarization with full field is represented in Fig. 8, the accelerated components D_1^2, D_2^2 harmonize in their sense of rotation with the magnetizing current. The $\frac{1}{4}\,\lambda$ plate reduces the circular polarization to plane polarization.

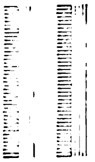

Fig. 8.

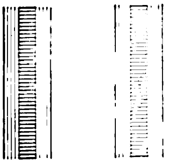

Fig. 9.

Viewing the flame in a plane perpendicular to the magnetic field the pole cores shown in Fig. 5 were used. The tripling can best be observed by means of the nicol; without the nicol the tripling could be faintly seen, but the strength of field necessary and the instrumental difficulties present make the observation of the phenomena far from satisfactory. The planes of vibration of the components are shown in Fig. 9. As no quarter wave plate is used this is the state of vibration in the ray of light itself.

It being apparent that no measurements could be taken without resorting to photography, the study was concluded at this point and work with the interferometer begun.

Summary. The following points may be noted as covering the first point aimed at in the work:

1. The magnetic separation of the sodium lines D_1 D_2, as given by a naked flame, cannot be distinctly observed at the temperature of the Bunsen flame, nor of the air-blast flame, nor even at the temperature of the oxygen gas flame, unless precautions are taken against spontaneous reversals.

2. The phenomenon can be much more satisfactorily observed parallel to the magnetic field when perpendicular to it, as the strength of field necessary to produce a pure (or visual) triplet is twice that necessary to produce the doublet.

3. There is a very perceptible time lag both when the magnet is excited and when the current is broken, during which period the lines show an inertia effect. This lag does not seem to be wholly due to the self-induction of the fields, but may be partially visual and partially *ionic.*

4. A field strength of at least 15,000 C. G. S. units seems to be necessary for satisfactory observation.

5. Spectra above the second order are too faint for good effects.

PHYSICAL LABORATORY,
UNIVERSITY OF WISCONSIN, April, 1899.

To be continued.

THE ELECTRICAL RESISTANCE OF LEAD AMALGAMS AT LOW TEMPERATURES.

BY GEORGE W. GRESSMAN.

THE purpose of the work described below was two-fold: to determine the temperature coefficient for resistance of some of the lead amalgams, and to locate their freezing points by means of the great decrease in resistance that occurs when the amalgam passes from the liquid to the solid state. Although the work is not yet completed, nor even entirely satisfactory, the results obtained seem of sufficient interest to warrant publication.

So far as the writer has been able to learn, no attempt prior to this has been made to determine the temperature coefficient for lead amalgams. G. Tammann[1] and D. Mazzotto,[2] however, have determined the freezing point of many lead amalgams. Unfortunately the articles by Mazzotto are not at my disposal, so that no comparison can be made with his results. Since the amalgams considered in this paper, with one exception, contain from four to twenty-five per cent. of lead, merely a qualitative comparison can be made with the results obtained by Tammann, as he experimented only with dilute amalgams. Still, the two sets of experiments do supplement each other and show that, in general, lead amalgams have a higher freezing point than pure mercury. Neither are lead amalgams the only exceptions to the general law that a solution has a lower freezing point than the solvent. Tammann found that cadmium, gold, and tin amalgams agree with the lead amalgams in this respect, while Van Bylert has shown that a rise in the freezing point occurs in solutions of antimony in tin; other writers have found similar results for naphthal in napthalene and carbazol in phenanthrene.

J. H. Van't Hoff[3] explains this rise in the freezing point as probably due to the fact that the dissolved substance and the solvent, are frozen out together. He says: "If the solvent upon being frozen is able to carry down some of the dissolved body, its vapor pressure diminishes and the freezing point correspondingly rises."

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[1] Zeitschrift für Physikalische Chemie, Band 3, 1889.

[2] Estratto d. atti d. Istituto Veneto d. sc. lettere ed. arti, 1892–93.

[3] Berichte d. deutsch. chem. Gesellschaft., 27, 1894.

Preparation of Amalgams.

The mercury that entered into the composition of the amalgams had been distilled and afterward cleansed by passing slowly first through concentrated sulphuric acid, and then through mercurous sulphate. Most of the lead was such as is ordinarily furnished as chemically pure. The percentage of lead in each amalgam was obtained by weighing the lead and mercury separately, and then computing the number of parts of lead in one hundred parts of the solution. Since the amalgams were, for the most part, prepared by fusing the lead and then adding the mercury after the lead had cooled down nearly to the point of solidification, the percentages given are subject to a slight error due to the vaporization of a small fraction of the mercury upon adding it to the heated lead.

All apparatus brought in direct contact with the amalgam or either constituent was cleansed with nitric acid, rinsed with alcohol, and thoroughly dried. Hence the amalgams can be considered free from impurities.

Apparatus.

The amalgams were contained in small U-tubes, varying from 1 to 2 mm. in diameter, being expanded at the ends sufficiently, however, to insert suitable terminals. Closely around this tube, from end to end, was wound a small coil of copper wire, by means of which the temperature was measured. After the tube had been filled with the amalgam, it was immersed in a test tube containing glymol. This prevented the temperature wire from coming in direct contact with the freezing mixture, and also protected against too sudden changes of temperature. The test tube was placed inside of another tube containing ether, into which solid carbon dioxide could be introduced in small quantities, thus gradually reducing the temperature below the freezing point of the amalgam. In order to preserve the freezing mixture, the tube containing it was enclosed by an air space filled lightly with mineral wool, to prevent convection currents, and then surrounded by a cooling bath of ice and snow.

MEASUREMENT OF RESISTANCE.

Since this work is concerned with resistance only so far as it depends upon the temperature, all resistances were measured in terms of an arbitrary standard by the fall of potential method. The standard resistance consisted of a small coil of German silver wire carefully wound with asbestos paper to protect it from sudden changes of temperature. As the temperature of the room never changed to any considerable extent during a single experiment, and in addition the temperature coefficient of the German silver wire is approximately one-tenth that of the copper wire, any variation in the resistance of the standard wire, due to the slight change in the temperature, was negligible.

A sensitive galvanometer, having a resistance about one thousand times as large as the unknown resistance, was used to measure the fall of potential. Hence by connecting the amalgam, the temperature wire, and the standard resistance in series, three galvanometer readings were sufficient to determine the temperature and resistance of the amalgam at any time; for the ratio of the fall of potential in the amalgam or temperature wire to that in the standard resistance will give the corresponding resistance in terms of this standard.

In order to eliminate the error which might arise from a lag in the amalgam taking on the temperature of the copper wire, the solid carbon dioxide was added in small quantities, so that measurements could be made during the process of cooling down as well as that of warming up. Furthermore, time was recorded in all cases, and an effort was made to have the warming up take place with the same rapidity as the cooling down. In general, the amalgams warmed up along the same curve as they had cooled down, but when there was any discrepancy the curve was drawn through the mean.

REDUCTION TO CENTIGRADE SCALE.

For the purpose of converting "copper degrees" into degrees centigrade, the temperature coefficient of a specimen of the copper wire was carefully determined in connection with Mr. A. H. Thiessen.[1] Tests were also made with the exact piece of wire used in measuring the temperature, and the average value obtained was .00405. At first an attempt was made to wind the copper wire on the tube, and then calibrate it by means of direct observations and the temperature coefficient. Thus the temperature corresponding to any resistance of the copper wire could have been read directly from a calibration curve, which would have rendered it a fairly convenient thermometer. This project was soon found to be impracticable, owing to the fact that the absolute resistance of the temperature wire was not constant, since it was frequently broken when cleaning the tubes, and also different lengths were used upon different tubes. Consequently, the temperature of the wire was accurately measured to tenths of a degree by a mercury thermometer at the beginning of each experiment. Then, knowing the temperature coefficient, the resistance of the wire at 0°C. can easily be obtained from the equation

$$R_t = R_0(1 + at),$$

from which

$$R_0 = \frac{R_t}{1 + at}$$

where R_t is the resistance of the wire at t°C., R_0 the resistance at 0°C., and a the temperature coefficient. The equation also shows that the change of resistance per degree centigrade is $R_0 a$. Hence, knowing the temperature coefficient and the temperature at the beginning of the experiment, any resistance of the copper wire can readily be reduced to the corresponding temperature on the Centigrade scale.

RESULTS.

After converting the resistances of the copper wire into degrees Centigrade by means of the above formula, the resistances of the amalgam were plotted as ordinates and the temperatures, as abscissas. The exact path of some of the curves may be questionable; yet, since several tests were made for each amalgam, sufficient points were obtained to locate the curves satisfactorily.

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[1] This value of the temperature coefficient differs about 1.2 per cent. from .00410, the value determined by Professors Dewar and Fleming.

Upon examining the curves in Figs. 1–4 we find at least two prominent breaks, which indicate a change of state in the substance.

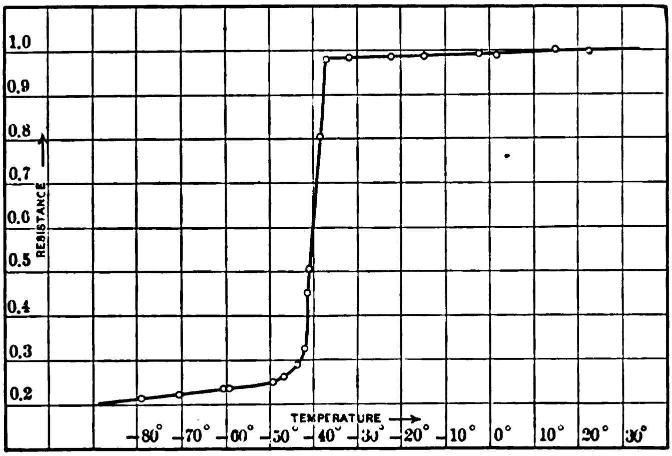

Fig. 1. Lead Amalgam, 4.2%.

The curves from 30°C. to the freezing point are but slightly inclined to the *X* axis. When the amalgam begins to freeze, however, the resistance drops off so suddenly that the curve runs at nearly right angles to its former direction. After the process of

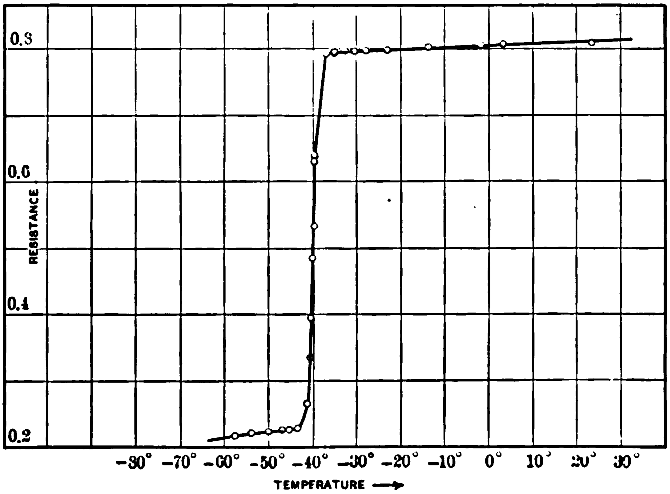

Fig. 2. Lead Amalgam, 7.1%.

freezing is completed, the curve continues nearly parallel again to the first portion. The resistance increased to such an extent upon freezing that in some cases the resistance of the solid amalgam was only one-fourth that of the liquid amalgam. Although the resistance drops off so suddenly, it was not possible to obtain a curve which indicated that the process of freezing took place at exactly one temperature, as in case of the conversion of water into ice. The

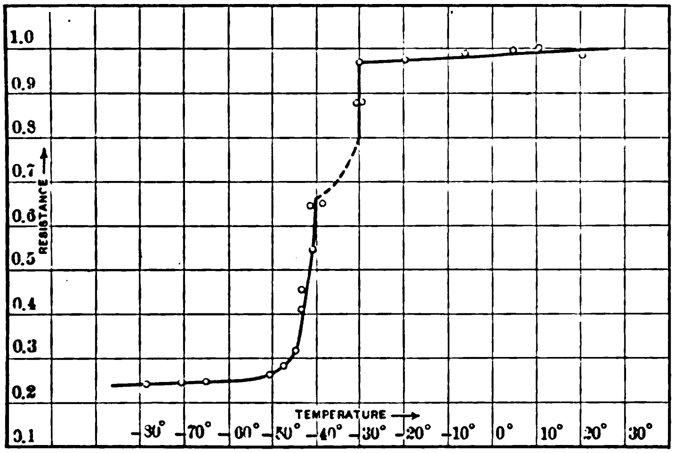

Fig. 3. Lead Amalgam, 11.2%.

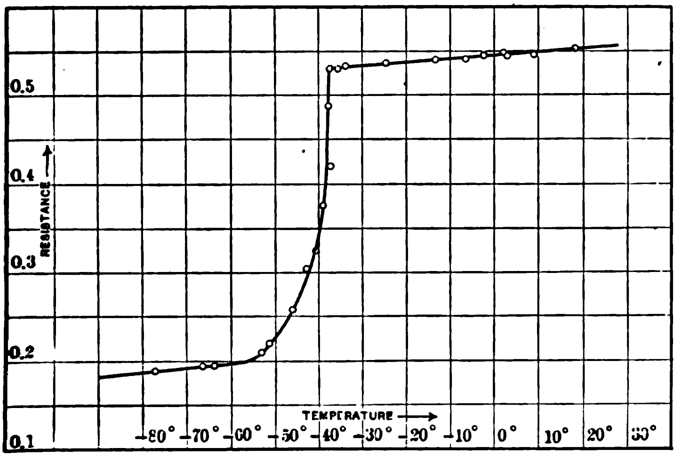

Fig. 4. Lead amalgam, about 25%. Viscid.

character of the curve suggests what might be termed a freezing point and a melting point; the former the temperature at which the amalgam begins to freeze, and the latter the temperature at which the entire specimen of the amalgam becomes a solid and, consequently, the temperature at which it begins to melt.

Melting Point.

The melting point seemed considerably the more difficult to locate. This was undoubtedly due to the fact that the solid amalgam gradually softened, causing the resistance to increase slowly at first and then, upon changing into a liquid, the resistance increased very rapidly until it was entirely converted into a liquid. That the amalgams did soften before melting is shown by the curves in Figs. 1–4, for the junction of the second and third portions of the curves would form a sharp angle as the first and second parts do, if the solid changes at once into a liquid. At present all that can be definitely stated in regard to the melting point is that usually the more lead the amalgam contains, the lower is the melting point. Thus the 25 per cent. amalgam, Fig. 4, began to melt at about — 55°, while the 4.2 per cent. amalgam, Fig. 1, melted at about — 45°.

Freezing Point.

The freezing point of the amalgam was easily located, since, in every instance, the resistance dropped off very suddenly as the amalgam began to freeze.

Table of Freezing Points.

Composition.	Freezing Points.	Average.
Pure Hg.	−38.8° C.	−38.8° C.
4.2% Pb.	−37.8	−37.7
	−37.6	
7.1% Pb.	−37.1	−37.0
	−36.9	
	−37.0	
11.2% Pb.	−30.2	−30.1
	−30.0	
Viscid Amalgam (25% Pb).	−37.6	−37.65
	−37.7	
Eutectic Alloy.	−40.0	−40.0
	−40.0	
2.4% Solder (Pb and Sm).	−33.1	−33.1

Upon examining the above table we find that, barring the extremes (where the amalgam is viscid and the eutectic[1] alloy), the rise in the freezing point depends upon the amount of lead in solution. Thus the 4.2 per cent. amalgam, Fig. 1, froze at — 37.7° and the 11.2 per cent. amalgam, Fig. 3, froze at — 30.1°.

NON-HOMOGENEITY OF THE AMALGAMS.

When the curve for the 11.2 per cent. amalgam was plotted, a peculiar break was found in the second portion of the curve. The amalgam plainly began to freeze at — 30.1°, but suddenly the temperature decreased to about — 40°. Although the test was repeated with similar results the exact path could not be determined. The curves for the other amalgams was then inspected, and similar breaks, only far less noticeable, were observed in them as is shown in Figs. 1, 2 and 4. This was taken to indicate that the amalgams are not homogeneous, and the second break marks the freezing point of one particular amalgam. Many alloys do possess a second freezing point which corresponds to the freezing point of the eutectic alloy. Hence if the lead amalgams do consist of a mixture, having different freezing points, it was supposed that the one having the lowest

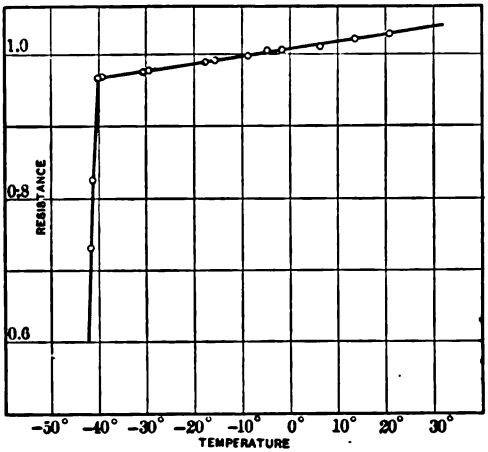

Fig. 5. Lead. Eutetic alloy.

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[1] The alloy having the lowest freezing point of any combination of the metals in question.

freezing point could be separated by freezing the larger part of the amalgam and then pouring off the small part, still liquid. Upon treating a dilute amalgam in this manner, the part remaining was found to freeze at — 40° C. This agrees almost exactly with the second break of the amalgams as noted above; thus corroborating the supposition made that the amalgams do possess a second definite freezing point.

Viscid Amalgam.

That the amalgams are not homogeneous is proven by the fact that whenever an amalgam contained 5 per cent. or more of lead, a thick mass separated out, which, however, could be converted into a pure liquid by trituration or heating. The more lead the amalgam contained the more of this viscid amalgam was formed. As near as could be determined without a chemical analysis, this amalgam contained about 25 per cent. of lead, and proved to be especially interesting. An amalgam was formed containing about 20 per cent. of lead from which the thick mass was and carried through a range of temperature from 90° to — 60°. The temperature curve obtained is approximately a straight line from 32° to — 37.7° (see Figs. 4 and 6). At the latter temperature theresistance of the amalgam dropped off very suddenly and it froze like the other amalgams. In the neighborhood of 32° the temperature coefficient became *negative* and remained negative and

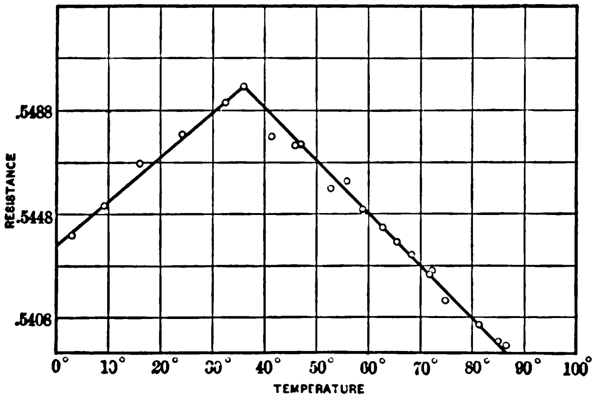

Fig. 6. Lead amalgam, about 25% lead. Viscid.

approximately constant to 90°. This seems especially remarkable, since the thick mass changed into a definite liquid during this rise in temperature, and from analogy with the other amalgams one would expect it to have a comparatively large temperature coefficient. Furthermore, for no temperature between — 10° and 90°, did its resistance differ more than one and one-half per cent. from that at ordinary temperatures.[1]

TEMPERATURE COEFFICIENTS OF OTHER AMALGAMS.

Owing to the fact that the resistance of nearly all the amalgams were at least one per cent. larger at ordinary temperatures, after having been cooled down or warmed up to any considerable extent, but little accurate work could be done in determining the temperature coefficients. The 7.1 per cent. amalgam was especially erratic. Frequently it could be cooled down 20° or more with scarcely a perceptible change in the resistance, but, upon allowing it to warm up, its resistance would increase quite suddenly about one per cent., after which it would again have a very small temperature coefficient. This change of resistance is probably due to a change in state of some portion of the amalgam.

While the temperature coefficients could not be accurately determined, observations were sufficient to show that all the amalgams investigated have a smaller temperature coefficient than pure mercury, and that for those containing a large percentage of lead, it is very small.

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[1] This experiment was repeated several times, with both increasing and decreasing temperatures, and three of the curves approximately coincided. The principal difficulty was to obtain the exact temperature at which the temperature coefficient changed from a positive to a negative value, and, also, the nature of the curve during this change. As the time for this work was limited, and the difficulty could not be easily overcome, since the change of resistance per degree was so slight, the sharp angle at 32°, indicated in Fig. 6, may not be absolutely correct. The general path of the curve, however, is unquestionably correct.

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THE WEHNELT ELECTROLYTIC BREAK.

By W. J. Humphreys.

THE Wehnelt electrolytic break, as is generally known, consists essentially of a small positive and a large negative electrode in an electrolyte of good conductivity. This arrangement, in series with the primary of an induction coil, under proper conditions, makes and breaks the circuit in a manner that often gives better results than can be obtained with any other known device.

The ease with which this break can be constructed and the excellence of its action caused me, as they have many others, first to try it and then to adopt it for most work with induction coils. Naturally, the instrument being new to me, I tried it under a variety of circumstances and, as in certain cases the conditions of my experiments and the resulting phenomena are different from those described by other observers, it seems worth while, owing to the importance of this remarkable break, to publish them.

The currents used were furnished, in nearly all cases, by a storage battery giving any voltage up to 75. A direct 110-volt circuit was also tried, but did not suit the experiments so well.

For the sake of clearness and ease of reference I shall group the observations under several heads.

ELECTRODES.

For negative electrodes I used sheets of lead, copper, amalgamated copper and coils of copper wire all with equal success. In all cases the positive electrode consisted of platinum sealed into the end of a glass tube, and connected to the circuit wire by means of mercury. This electrode was adopted not only because it is so easy to make but also because, if to last, it must be able to resist the action of oxygen even in the form of ozone. The wires used varied from one-fourth to one millimeter in diameter, and from one and a-half to twelve millimeters in length. In one case the end of the wire was fused to a globular form, in another the wire was looped, both ends being sealed into the glass; in still another the electrode was a piece of sheet platinum sealed across the end of a glass tube, its edges being covered by glass. The usual form, however, was a short straight wire. In some cases the electrode pointed down, in others horizontally, in still others up. In all cases, however, the character of the action was quite the same, though electrodes, with larger surface, demanded larger currents and higher voltage to put them in action. Under the same conditions the larger electrodes produced less frequent breaks and gave discharges of larger volume from the secondary than did smaller electrodes. The distance, within wide limits, between the positive and negative electrodes has but little effect on the general result. The same is true in regard to the position of the positive electrode, provided the gases formed around it can be replaced easily and quickly by the electrolyte.

In making these electrodes I first sealed a bead of blue enamel glass on the wire, and that in turn to a tube of soft German glass. When made in this way they seem to stand the roughest sort of usage without cracking or giving any other kind of trouble.

In some cases at least that part of the platinum which is in the mercury becomes very nicely amalgamated, a phenomenon I had not anticipated nor do I know yet the exact conditions under which it happens.

After considerable use that part of the platinum which is in the electrolyte becomes thickly pitted, giving it, under the microscope, a beautiful frosted appearance. This is due doubtless to an arc discharge, to be described later on, formed between the platinum and electrolyte each time the circuit is being broken.

ELECTROLYTES.

Those electrolytes that conduct best give the best results with the break. They also give the most vigorous action when their concentrations are such as to give the best conductivities at the temperature at which they are used. Thus for 18°C. the best mixture of sulphuric acid and water is about 30 per cent. acid and 70 per cent. water, though the action is still quite good both with more dilute and more concentrated acid.

Hydrochloric acid and nitric acid are both quite as good, for a time at least, as sulphuric, probably even a little better, since, according to Kohlrausch, they are slightly better conductors.

Of the various salts tried ammonium chloride is one of the most interesting in its action. Under the same conditions it does not give as long nor so heavy sparks as does sulphuric, nitric or hydrochloric acid; still the action is very good and the rapidity of the break is much greater, giving under suitable conditions a note both clear and shrill.

The case of melted ammonium nitrate is also interesting, showing

ELECTROLYTES TESTED.

Name.	Action.	Remarks.
Sulphuric acid	Excellent	Good conductor
Hydrochloric acid	Excellent	Good conductor
Nitric acid	Excellent	Good conductor
Boric acid	None	Very poor conductor
Oxalic acid	None	Very poor conductor
Hydrobromic acid	Slight	Weak solution used
Phosphoric acid	Fair	Moderate conductor
Sulphurous acid	None	Very poor conductor
Ammonium sulphate	Slight	Rapid break
Copper sulphate	None	Very poor conductor
Sodium sulphate	Slight	Poor conductor
Acid sodium sulphate	Slight	Poor conductor
Ammonium chloride	Well	Very rapid break
Calcium chloride	Slight	Rapid break
Lithium chloride	Slight	Poor conductor
Sodium chloride	Fair	Moderate conductor
Potassium chloride	Fair	Rapid break
Strontium chloride	Slight	Poor conductor
Zinc chloride	Fair	Moderate conductor
Ammonium nitrate	Fair	Rapid break
Ammonium nitrate, fused	Slight	Action irregular
Sodium nitrate	Fair	Rapid break
Bromine water	None	Very poor conductor
Potassium bromide	Slight	Poor conductor
Sodium bromide	Slight	Poor conductor
Sodium carbonate	Slight	Rapid break
Acid sodium carbonate	Slight	Rapid break
Sodium bichromate	Fair	Moderate conductor
Caustic soda	Fair	Ropid break
Potassium iodide	Fair	Moderate conductor

as it does that the break may act, though irregularly and feebly, at a relatively high temperature and without the presence of water.

The above table contains a list of the substances tested. Of course this could have been greatly extended, but it did not seem necessary.

To Prevent Acid Spray.

Dilute sulphuric acid seems to be the best electrolyte for general use with the break, and while the acid does not evaporate or give off, during the action of the break, any objectionable gases, still if an open vessel is used, the bursting of the bubbles soon fills the air with acid spray which is both disagreeable and injurious to apparatus. Of course, several methods may be devised to prevent this, but I have found a covering of paraffin oil to be perfectly satisfactory. The electrodes may be dipped back and forth through the oil without its adhering to them sufficiently to prevent their prompt action when the circuit is closed. This oil covering has the further advantage of preventing any change in bulk of the electrolyte, due to evaporation or absorption of water, while the break is not in use.

Glow About Anode.

During the action of the break there is a more or less brilliant glow about the anode. When there is no glow the current is not interrupted. The converse of this, however, when there is no break there is no glow, is not true, for during the high resistance stage, to be described below, when there is no breaking of the circuit, there is a feeble but continuous glow on a part of the anode, stationary at points and apparently coming and going along rough places in a manner that reminds one of brush discharges.

On examining the glow with a revolving mirror, when the break was running nicely, it was found to be intermittent, the light appearing at uniformly spaced intervals, with, so far as I could tell, total absence of light between them. Rapid revolving seemed only to increase the spaces between the flashes of light and not to stretch out the light at all appreciably.

The spectrum of this light contains, certainly, lines of hydrogen, oxygen and other things, owing to the nature of the electrolyte.

Those electrolytes containing sodium give a brilliant yellow glow, while calcium chloride gives a brilliant red glow and a beautiful spectrum of calcium. Similar phenomena are true of the potassium and lithium salts. The pinkish glow in dilute sulphuric acid is due mainly to the brilliant red line of hydrogen.

The fact that these glows give line spectra of hydrogen and oxygen is well-nigh positive proof that the light is due to an electric discharge through these gases or compounds of them surrounding the electrode, since the only certainly known method of obtaining line spectra from small amounts of hydrogen and oxygen is by means of passing an electric discharge through them.

EFFECT OF MAGNETIC FIELD.

On placing the break in a strong magnetic field so that the lines of force were at right angles to the current, it was found that its action was greatly disturbed. When the voltage was just high enough to run it nicely under ordinary circumstances, it would not act at all in a magnetic field; when, however, the voltage was high it would act, but very irregularly; the glow seemed to be blown out almost as soon as formed and then to begin again very soon, once more to be blown out and so on indefinitely.

EFFECT OF CHANGE IN HYDROSTATIC PRESSURE.

It does not take a very great change in hydrostatic pressure to produce a noticeable change in the pitch of the note given by the break and in the character of the spark from an induction coil connected with it. An increase in the pressure lowers the frequency of the discharge and produces heavier sparks, while at very low pressures the action of the break is quite feeble.

GASES GENERATED.

The kind and quantity of the gases generated by the positive pole of the Wehnelt break are different from those obtained during the process of quiet electrolysis, at least this is the case with the substances examined, sulphuric, nitric and hydrochloric acids and ammonium chloride. The proportions vary with the size of the electrode, voltage of the current, and other conditions. The negative pole gives off hydrogen alone. When the break is running best in a solution of sulphuric acid the volume of the gas from the positive pole is very nearly equal to that from the negative, instead of being only about half as great as it is in the case of quiet electrolysis. This gas consists of a mixture of hydrogen and oxygen. After exploding it the remaining gas is oxygen alone, but not nearly enough to account for the hydrogen from the negative pole on the assumption that the gases come from the decomposition of the water only. I used long eudiometers made from the same glass tube of about two centimeters internal diameter and a typical case gave the following result:

Before explosion,	Length of gas column over negative pole	28.5 cm.
	" " " " " positive "	22.0 "
After explosion,	Length of gas column over negative pole	28.5 cm.
	" " " " " positive "	7 "

A number of sparks were passed through the gas in each eudiometer, but the gas over the negative pole, being hydrogen alone, did not change in volume, while that over the positive pole exploded quite vigorously. The mixture of hydrogen and oxygen can be accounted for by the decomposition of the water by the arc discharge that forms the flashes of light about the positive pole. More or less ozone and persulphuric acid are also to be expected about the positive pole, but whether these can in any way fully account for the relatively small volume of oxygen left after the explosion is an interesting question which at present I cannot answer.

I also found the gases from the positive pole in nitric acid to explode, but not very violently. The residue was oxygen. In the case of hydrochloric acid the explosion was quite violent, probably between hydrogen and chlorine, at any rate the remaining gas consisted largely or entirely of oxygen.

I did not risk collecting much gas from ammonium chloride, but the small amount produced a violent explosion that jerked the eudiometer from its support and broke it. Probably in this case not only hydrogen, chlorine and oxygen were present, but also a small amount of nytrogen trichloride, one of the most violent explosives known.

PLACE OF CLOSING CIRCUIT.

It has been affirmed and denied that the place of closing the circuit has anything to do with the action of the break. My own experience convinces me that with a given electrode, external resistance, which should be low, etc., it depends upon the voltage. If the voltage is low, just enough to run the break properly, then it starts at once no matter where the circuit is closed. If it is closed by dipping the electrode into the acid it starts up as soon as it touches the liquid, though the action, so long as the electrode is kept at the surface is somewhat irregular and the liquid is spattered about. On pushing the electrode below the surface the action goes on nicely. If however the voltage is high then the electrode must be put into the acid first and the circuit closed on the outside. If closed by dipping the electrode into the acid the high resistance state, to be described below, is formed at once, and this too no matter how quickly or slowly the electrode is immersed. With intermediate voltages the action is uncertain. Sometimes the breaking begins at once, at others the high resistance state is formed which, if left alone, may or may not give way to breaking.

VOLTAGE REQUIRED.

While working with a small electrode I found that the current rose with the voltage to a maximum, the value of which depended, among other things upon the self induction of the circuit, and then decreased with further increase of the voltage. The break however did not appear to run regularly at the higher voltages, as it gave a somewhat harsh or ragged sound. The particular electrode used, with a surface of only four or five square millimeters gave a maximum current in sulphuric acid with about 50 volts.

TYPES OF ACTION OF ELECTRODES.

The positive electrode has three stable conditions of activity that differ very greatly from each other. I shall call these for convenience the high resistance stage, which allows a very small current to flow, the stage of quiet electrolysis which carries a much larger current, and finally the stage of regular breaking which gives the largest current of all. These do not merge gradually the one into another, but change abruptly. The high resistance stage is characterised by minute but steady glows on points and along rough edges, a small current, considerable heating, and a peculiar frying noise. The stage of quiet electrolysis by absence of light and noise, together with a relatively large current. The regular breaking stage by a clear-cut note of constant pitch, a bright but intermittent glow and a large average current.

Resistance in Circuit.

An adjustable resistance of considerable range in series with the break gives some interesting results. When the resistance is large the current produces only quiet electrolysis. On decreasing the resistance the electrolysis increases till a certain current, depending largely upon the size of the electrode, but not upon the voltage, is reached when irregular breaking and a decreased average but variable current is obtained. On still further decreasing the resistance the break usually becomes steady and the current quite large. Sometimes, however, instead of becoming steady the break gives way to the high resistance stage which remains comparatively unaltered through a wide change in the external resistance. Breaking the circuit and connecting it again will now start the break to running regularly if the resistance is small, or begin quiet electrolysis if the resistance is large. On sufficiently increasing the external resistance the high resistance stage disappears abruptly with a peculiar click, and is followed by quiet electrolysis.

When the voltage is comparatively low the high resistance stage often ceases of itself with a sudden outburst of breaking, this in turn may soon be followed by a renewal of the high resistance stage and so on indefinitely but irregularly. In no case, however, have I seen the full breaking stage give way to quiet electrolysis nor the reverse.

Self-induction in Circuit.

An increase in the self-induction of the circuit lowers the frequency of the break. With very small self-induction the break can be obtained only with correspondingly small anodes. A decrease in the self-induction also causes an increase in the average current. I tried, with two coils, and with different electrodes and various voltages from 35 to 75 the effect of short-circuiting the primary, and found in every case, where the break continued to run with a clear note, that its frequency and the average amperage both increased when the coil was cut out. This increase of current is just the reverse of that described by H. Pellat, C. R., 12, CXXVIII. With relatively large electrodes, however, the break often did not continue to run smoothly when the coil was cut out, and occasionally changed in a little while from irregular breaking to the high resistance stage. In these cases, that is when on cutting out the coil the break became irregular, the current at the same time became exceedingly variable and its average value decreased.

Formation of the Gas.

When the break is running the bubbles of gas are hurled from it with considerable velocity. This I found most marked with the anode of sheet platinum. When sulphuric, nitric or hydrochloric acid is used the bubbles are relatively large, while ammonium chloride, sodium nitrate and others, especially those that give high frequency, produce some bubbles of fair size together with vast numbers of exceedingly minute ones that give the liquid a milky appearance.

Shunt to Break.

A shunt to the break when its resistance is large carries a greater current when the break is running than it does even when it is the only resistance in the circuit. This is very nicely shown with an incandescent lamp in the shunt. If the resistance of the shunt is gradually decreased the break begins to act irregularly, and soon ceases entirely.

Electrodes in Series.

When a large and a small electrode are connected in series, and the voltage is suitable, the large one gives quiet electrolysis while the small one produces the breaking quite the same as when it alone is in the circuit, except in so far as its frequency is decreased by the resistance of the large electrode. When both electrodes are approximately the same size both may break the circuit, but the action is irregular and does not give a clear note.

Uses.

It is not my purpose to discuss the various uses of the Wehnelt break, which seem to be nearly all those of any other, but simply to mention one or two things I have come across in using it practically. So far I have not obtained very satisfactory results with it in connection with a twelve-inch coil wound for a direct 110-volt circuit, nor have I examined the conditions sufficiently to know just what the trouble is.

In using the break on an ordinary Ruhmkorff coil for a large X-ray tube, I obtain best results with a break of moderate frequency, about three hundred per second. Higher frequency causes the tube to light up throughout, nor is this obviated by the introduction of a spark gap. With a suitable sized electrode, however, the action is most satisfactory for both fluoroscopic and radiographic work. Thus with an exposure of only two seconds I secured a fully exposed and excellent radiograph of a man's hand, while with the fluoroscope it is perfectly easy to see through any part of the body of an adult, the light meanwhile being quite free from all flicker.

Action of Break.

I do not claim the following explanation to be either complete or wholly free from error; it is only that which my experiments have suggested to me.

With moderately high voltage the current grows quickly though gradually on account of the self-induction, till the electrode is completely sheathed by a layer of gas—chiefly or entirely oxygen. This gas abruptly breaks the circuit and the self-induction sends an electrical discharge across it, giving rise to the light and at the same time decomposing some of the liquid, giving among other things oxygen and hydrogen. The sudden freeing of these gases and the heat of the discharge together produce an explosive effect, as shown by the noise and the violence with which the bubbles are thrown off. Probably no part of the hydrogen and oxygen set free is again recombined. The flash seems due to a single discharge, certainly there can be no oscillations, and as some of the liquid is undoubtedly decomposed, as shown by the gases collected, it does not seem clear how the same discharge can also cause them to recombine;

at any rate a part of the gas remains decomposed, and it seems highly probable that this would not be so if a single discharge could decompose water into its elements and at the same time trail after itself a recombination of them.

The explosion over, the liquid drops back onto the electrode, cleared by the discharge of any polarization, and the process renews itself.

According to this, disturbance from a magnetic field is to be expected, so too are the effects of changes in hydrostatic pressure, the actions of large and small electrodes, or high and low voltages, of changes in resistance, self-induction, and other things as already described.

I am very sorry that I cannot, owing to want of apparatus, examine the break with a rapid curve tracer, such as the one described by Mr. H. J. Hotchkiss in the Physical Review for March, 1899.

I venture to say, however, that such a trace will show the current to rise gradually from zero to a certain maximum and then to drop very nearly abruptly to zero again, and so on indefinitely. At break of current the voltage will probably rise quite high, to an extent dependent, of course, among other things, upon the self-induction of the coil. At make of current the voltage, probably differing but little by this time from that of the battery, may fall slightly and then continue very nearly constant till the next break.

I have not attempted to give the bibliography of the break, partly because my work has been independent of other observers, but chiefly because it is too soon, owing to the large number of short articles appearing on it, to make the bibliography at all complete or serviceable.

Rogers Physical Laboratory, University of Virginia, April 14, 1899.

THE HYDROLYSIS OF STANNIC CHLORIDE.[1]

By William Foster, Jr.

MANY compounds when in aqueous solution are acted upon by the water, some of the atoms or radicals of the compounds being replaced by the hydroxyl (OH) group. This familiar phenomenon is termed *hydrolysis*. Such bodies as stannic chloride and ferric chloride belong to this class of compounds. The rate at which a body undergoes hydrolysis is largely regulated by the amount of water present in proportion to the compound, a large amount of water facilitating hydrolysis. Stannic chloride is one of the most instructive examples of this class of compounds, for the rate at which it suffers hydrolysis in solution of medium concentration is so gradual that the observer has ample time to determine the condition of the solution as the reaction progresses, and at the same time final equilibrium is reached without having to wait too long.

In determining the resistance of a freshly-prepared solution of half-normal $SnCl_4$, I observed that the bridge-reading was not constant, but that it changed in such a manner as to indicate that the conductivity of the solution *increased*. As the change in the conductivity was so gradual, it at once suggested the idea of measuring the conductivity of a solution from time to time until it should become constant. A number of such measurements have thus been made.

It will be remembered that Loomis[2] found very large and surprising values for the molecular depression of the freezing-points of aqueous solutions of this salt. Thus while the maximum values found by him in other salts did not exceed 5.5, the value for $SnCl_4$ was found to be 12.61 for $m = 0.04$ ($m = 0.01$ according to Loomis).

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[1] Part II. of thesis presented to the faculty of Princeton University for the degree of Doctor of Philosophy.

[2] Phys. Rev., Vol. IV., No. 22, 1897, pp. 278 and 294.

[illegible] this salt as such could not account
[illegible] present work was undertaken at the
[illegible] with the hope of finding some
[illegible] from the behavior of the compound

[illegible]TORICAL.

[illegible] to the ancient chemists. It was
[illegible] back as 1605, and it was early termed
[illegible] has been known for some time that it
[illegible] ous solutions, but that it undergoes
[illegible] of tin.

[illegible] for some time that tin forms at least two
[illegible] sometimes termed stannic and metastannic
[illegible] acids, respectively.

[illegible] present century, observed the difference
[illegible] thought that the stannic acid was ob-
[illegible] with alkalies; and the metastannic acid,
[illegible] acid.

[illegible] of the very conflicting opinions held
[illegible] composition of these bodies:
[illegible] into metastannic acid by desiccation.
[illegible] dry state.[1] (Berzelius, Graham.) Stannic
[illegible] alteration. Its composition is the same
[illegible] Neumann, Schiff, Lorenz.)[2]
[illegible] metastannic acid dried in a vacuum is $(SnO_2)_5$.

[illegible] not exist: they are stannates with excess
[illegible]) There exist between these bodies two
[illegible] gnon.)
[illegible] stannic hydrates, each of which behaves
[illegible] corresponding salts.
[illegible] degrees of hydration between H_2SnO_3 and
[illegible] explanation of their difference has been
[illegible] with such compounds as NH_4NO_3 and

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[1] [illegible] Compt. Rend., 125, 651; 1897.
[2] [illegible] Chem., 9, 376; 1895.

Na_2SO_4; it yields H_4SnO_4 when dried in the air; and H_2SnO_3 when dried over H_2SO_4. Stannic hydrate is slightly soluble in water, and dissolves readily in dilute mineral acids. (Roscoe.)[1]

Acid solutions of SnO_2 treated with alkalies, precipitate $Sn(OH)_4$ (stannic hydrate), also known as stannic acid. It is a colloidal substance having several modifications which are identical in composition. The various hydroxides have also a different appearance and behave differently with reagents. There is a difference between ordinary stannic acid and metastannic acid. Stannic acid is precipitated from $SnCl_4$ by treating it with alkalies, and metastannic acid is obtained by treating tin with HNO_3. Metastannic acid is insoluble in water in the presence of HCl. (Mendeleeff.)[2]

The cause of difference is due to polymerization.

The ordinary acid corresponds to SnO_2; and the metastannic acid, to Sn_5O_{10}. (Fremy.)

The following opinions are entertained in regard to the change which $SnCl_4$ suffers in aqueous solution: Dilute HCl, especially when boiling, changes the ordinary hydrate which is first formed into metastannic acid. On this depends the formation of a white precipitate, stannic hydrate, in dilute aqueous solutions. (Mendeleeff.) When solutions of stannic acid in HCl (identical with solution obtained by the action of H_2O on $SnCl_4$) are allowed to stand, the stannic acid slowly undergoes conversion into metastannic acid, which usually separates out as an opalescent precipitate. (Roscoe.)

The dilute solution of $SnCl_4$ gradually decomposes, yielding HCl and metastannic acid. (Watts' Dict. of Chem.)

The solution of $SnCl_4$ diluted to a certain point yields *stannic hydrate* as well as metastannic acid. (Casselman.)

Stannic chloride boiled in water yields H_2SnO_3 and HCl, — $SnCl_4 + 3H_2O = H_2SnO_3 + 4HCl$.

Probably the following reaction takes place first:

$$SnCl_4 + 4H_2O = Sn(OH)_4 + 4HCl.$$

$Sn(OH)_4$ loses a molecule of H_2O and forms H_2SnO_3, stannic acid. (Remsen.)

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[1] Treatise on Chem., Vol. II., 1897.

[2] Treatise on Chem., Vol. II., London, 1897.

APPARATUS.

The apparatus employed in measuring the conductivity of solutions of $SnCl_4$ was the same as that described in Part I. of this thesis, with the addition of a thermostat.[1] The measurements in the thermostat were conducted at 28° C. It was easy to regulate the thermostat so that the temperature varied only a small fraction of a degree (usually much less than $\frac{1}{10}$) for hours at a time. Other measurements were made at 18°.

THE PREPARATION OF THE COMPOUND.[2]

To prepare $SnCl_4$, c. p. tin was melted in a retort, and a strong, dry stream of chlorine was conducted into it, whereby $SnCl_4$ was formed. It is a strongly-fuming liquid; consequently a long condenser was connected with the retort, and the salt condensed in a pair of Woulf bottles surrounded by an ice bath. The liquid was transferred to a fractionation flask and some tin foil added to unite with the dissolved Cl. The $SnCl_4$ was distilled, treated with tin foil again, and the distillation repeated until the liquid became colorless and had a constant boiling-point.

The liquid was preserved in a well-stoppered flask to prevent access of aqueous vapor. The normal solution was prepared by first making up a solution of specific gravity 1.0927. This was analyzed, the Sn being determined as SnO_2 by precipitation as stannic hydrate by means of a strong solution of NH_4NO_3, with the following results:

I.	5 c.c.	of the	solution	gave	0.3551	g.	SnO_2
II.	"	"	"	"	0.3561	"	"
		mean value			0.3556		

This corresponds to 123.16 grams of $SnCl_4$ per liter. Accordingly, 288.23 grams of this solution were diluted to 500 c.c.

10 c.c. of this gave 0.3752 grm. SnO_2, which corresponds to 65.07 grms. of $SnCl_4$ per liter. 64.975 grams are required for a normal solution. This indicated that the solution was 0.15% too

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[1] Ostwald, Physiko-Chem. Messungen, Leipzig; 1893, p. 70.

[2] Chemische Präparatenkunde, Bender and Erdmann, Stuttgart.

strong, so it was diluted to normal strength. This solution was employed in preparing most of the dilute ones. For a few of the more concentrated solutions, the ordinary c. p. salt was employed. The strength of the solutions, other than those prepared by direct dilution from a solution of known strength, was determined by chemical analysis in some cases; and in others, by comparison of their sp. gr. with the sp. gr. of the solutions of known strength.

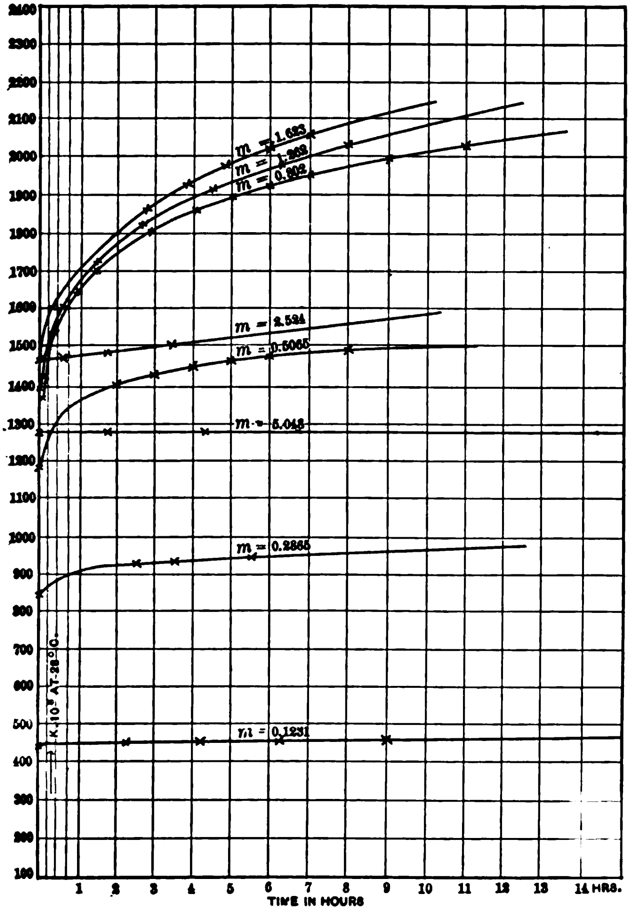

Fig. 1.

Curves for $SnCl_4$.

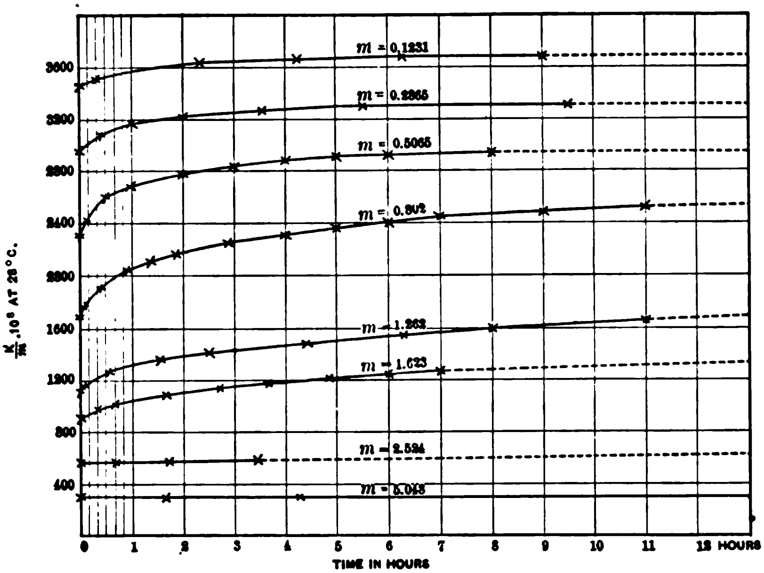

Fig. 2.

Curves for $SnCl_4$.

THE MEASUREMENTS.

The Normal Solution.

The specific gravity of the normal solution of $SnCl_4$ was found to be 1.0496 $(\frac{18^\circ}{4^\circ})$. The solution was prepared from the pure liquid salt on November 12, and the values of $\frac{K}{m}$ (equivalent-conductivity) at 18° are given below :

$\frac{K}{m} \cdot 10^8$ at 18°C.	
November 14,	1805
" 18,	2040
December 23,	2265
February 21,	2340

This solution remained *clear*, and stood in a room whose temperature averaged about 15°. It is thus seen that the normal solution changes more rapidly during the first few days after its preparation, and that it would be months in coming to its maximum conductivity. Solutions from m=0.0001 to m=0.01 were prepared from the normal solution as in the first part of this work.[1] Two series of measurements were carried out. The results are given below. The concentration m in *gram-equivalents* is given in the first column; the specific conductivity at 18° (Hg at 0°=1) is given in column 2; and the equivalent-conductivity $\frac{K}{m}$ is found in column 3.

	First Series.		Second Series	
m	$K_{18}10^{10}$	$\frac{K}{m} . 10^8$	$K_{18} . 10^{10}$	$\frac{K}{m} . 10^8$
Water.	(1.83)	——	(2.10)	——
0.0001017	37.06	3460	32.52	2990
0.0002032	71.08	3400	67.35	3210
0.0006077	205.07	3350	204.61	3320
0.001009	344.2	3390	342.40	3370
0.002015	683.7	3390	685.70	3390
0.006024	2010.5	3330	2010.5	3330
0.01000	3300.0	3300	3313.0	3310

The measurements of the first series were made as rapidly as possible, while more time was used in obtaining the second series. It is seen, then, in the *extremely dilute* solutions that the equivalent-conductivity *decreases.* The same phenomenon is characteristic of acids and bases.

When m = about 0.001, the value of the two series come together, however, and solutions of such strength change very little with time. To ascertain just how much a solution in which m = 0.001 does change, or how much time it requires to come to final equilibrium, one was prepared directly from the normal solution, with the following result:

m = 0.001	$\frac{K}{m}$.10⁸ at 18°
Feb. 6, 11 a. m.	3100
" " 2:30 p. m.	3440
" " 5 "	3440

It is thus seen that this solution reaches its maximum conductivity within two or three hours. It changes very rapidly at first.

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[1] Physical Review, May-June, 1899.

For $m = 0.01$, the freshly-prepared solution gave $\frac{K}{m}.10^8 = 3300$, and this soon reached a maximum value of 3475. Some interesting conclusions may be drawn from the facts now before us, namely:

1. The final conductivities of the solutions of $SnCl_4$ $m = 0.01$ and 0.001 are almost identical with those of HCl of the same concentrations.

m	$SnCl_4$	HCl.
0.001	3440	3520 } $\frac{K}{m}.10^8$
0.01	3475	3470

For $m = 0.01$, these values are practically identical.

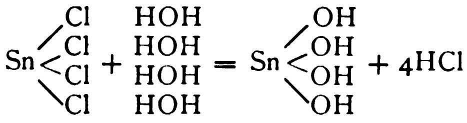
2. For the most dilute solutions of $SnCl_4$, the values of $\frac{K}{m}$ become smaller. The same is true of HCl in dilute solutions.

3. The *dilute* solutions of $SnCl_4$ soon come to equilibrium, that is, the hydrolysis of the salt is soon completed.

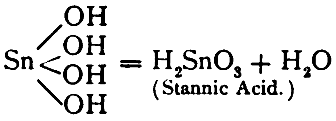
It is worthy of note that these dilute solutions yield a white, amorphous precipitate. It is a well-established fact that many chlorides undergo hydrolysis to a greater or less extent, generating at the same time HCl; and it has been known for some time that a large quantity of water, especially when heated, causes an aqueous solution of $SnCl_4$ to precipitate a compound variously termed "stannic hydrate," "stannic acid" and "metastannic acid."

From the data and facts given above, it would seem that the stannic compound formed by the action of the water, even before the separation is complete, is not dissociated to any considerable degree; but that the conductivity of the solution depends almost entirely upon the HCl that is generated.

The following reaction perhaps takes place:



(Stannic Hydroxide.)



$$Sn\begin{cases}OH\\OH\\OH\\OH\end{cases} = H_2SnO_3 + H_2O$$

(Stannic Acid.)

$5H_2SnO_3 = H_{10}Sn_5O_{15}$ (insoluble metastannic acid).

It is here assumed that the $SnCl_4$ first decomposes, forming stannic hydroxide, and generating four molecules of HCl. The $Sn(OH)_4$ then loses a molecule of water, forming stannic acid, and then the H_2SnO_3 molecule condenses to form $H_{10}Sn_5O_{15}$ (metastannic acid).

This is an insoluble, colloidal body which gradually settles down as a white precipitate.

100 c.c. of the $\frac{n}{100}$ $SnCl_4$ solution (*i. e.*, the clear supernatent liquid) was found to be in exact balance with 10 c.c. of $\frac{n}{10}$ NaOH, thus showing that an equivalent amount of HCl was in fact generated.

Measurements were now made on the $\frac{n}{10}$ solution of $SnCl_4$, with the following results :

	$\frac{K}{m} \cdot 10^3$
Feb. 6, 9:30 a. m.	3125
" " 5 30 p. m.	3154
" 7, 2:00 "	3159
" 21,	3218

The last value remained constant from this time. The value of $\frac{n}{10}$ HCl is 3290.

The $\frac{n}{10}$ $SnCl_4$ (the clear supernatent liquid) was balanced against $\frac{n}{10}$ NaOH, and was found to be 1.6% weak.

Measurements were next made on a number of solutions at 28° with results as follows :

SOLUTION NO. I. $m = 1.623$.

Some of the liquid $SnCl_4$ (about 10 c.c.) was quickly added to about 200 c.c. H_2O. The temperature was quickly brought to 28°, $SnCl_4$ and the resistance of the solution determined without delay. Time of bringing liquid $SnCl_4$ and H_2O together, 12 m.

Time.	Interval in min.	K 10^8 at 28°	$\frac{K}{m}$ $.10^8$
Jan. 11, 12.05 p m.	0	1485	915
12.10 "	5	1532	944
12.25 "	20	1608	991
1:45 "	100	1757	1083
2:45 "	160	1854	1142
3:45 "	220	1921	1183
5:00 "	295	1981	1221
6:05 "	360	2018	1243
7:05 "	420	2049	1262
Jan. 30.	19 days	2791	1719

II. $m = 0.802$.

Time of bringing $SnCl_4$ and H_2O together, 10.31 a. m.

Time.	Interval in Min.	K. 10^8 at 28°	$\frac{K}{m}$. 10^8
Jan. 13. 10:35 a. m.	0	1377	1717
10:40 "	5	1440	1796
11:00 "	25	1559	1944
11:30 "	55	1646	2052
12:01 p. m.	86	1706	2127
12:31 "	116	1748	2179
1:31 "	176	1806	2252
2:35 "	240	1857	2315
3:35 "	300	1892	2359
4:35 "	360	1922	2396
5:35 "	420	1948	2429
7:35 "	540	1989	2480
9.35 "	660	2021	2520
Jan. 30	17 days.	2251	2807
Feb. 21	39 days.	2293	2860

2860 at 28° = 2460 at 18°.

III. $m = 0.5065$.

$SnCl_4$ and H_2O came together at 1:50 *p. m.*

Time.	Interval in min.	$K \cdot 10^8$ at 28°	$\frac{K}{m} \cdot 10^0$
Jan. 18. 1:45 p. m.	0	1183	2335
2:01 "	7	1246	2460
2:24 "	30	1321	2608
2:54 "	60	1362	2689
2:54 "	120	1405	2773
4:54 "	180	1434	2831
5:54 "	240	1451	2878
6:54 "	300	1466	2894
7:54 "	360	1475	2912
9:54 "	480	1488	2937
Jan. 19	1 day	1507	2975
" 30	12 days	1542	3044
Feb. 21	34 days	1562	3084

3084 at 28° = 2660 at 18°.

IV. $m = 0.2865$.

$SnCl_4$ and H_2O came together at 8:59 a. m.

Time.	Interval in min.	$K \cdot 10^8$ at 28°	$\frac{K}{m} \cdot 10^8$
Jan. 17. 9:03 a. m.	0	841	2935
9:07 "	4	857	2957
9:30 "	27	887	3096
10:03 "	60	906	3162
11:03 "	120	921	3215
12:33 p. m.	210	930	3246
2:33 "	330	940	3281
6:30 "	567	946	3302
Jan. 30.	13 days.	957	3340

This solution on standing gave a white precipitate. Nos. I., II. and III. remained *perfectly clear.* The value of m in every case was determined at 15° C.

Four series of measurements were next made, starting with a very concentrated solution ($m = 5.048$). The c. p. $SnCl_4$ (crystalline) of Eimer and Amend was employed. It was analyzed at 15° C.

These solutions will be designated as A, B, C and D.

Solution A. $m = 5.048$.

Time.	Interval in min. min.	$K . 10^8$ at 28°	$\frac{K}{m} . 10^8$
Jan. 14. 9:50 a. m.	0	1272	252
11:32 "	102	1274	252
4:33 p. m.	260	1272	252
Jan. 30	16 days.	1281	256

252 at 28° = 207 at 18°.

B. $m = 2.524$.

Time.	min.	$K . 10^8$ at 28°	$\frac{K}{m} . 10^8$
Jan. 15. 4:05 p. m.	0	1461	579
4:45 "	40	1468	582
5:45 "	100	1479	586
7:30 "	205	1501	594

C. $m = 1.262$.

Time.	min.	$K . 10^8$ at 28°	$\frac{K}{m} . 10^8$
Jan. 16. 9:29 a. m.	0	1409	1116
9:33 "	4	1458	1155
10:00	31	1598	1266
11:00 "	91	1727	1369
12:00 m.	151	1815	1438
2:00 p. m.	271	1913	1516
3:45 "	376	1971	1562
5:30 "	481	2025	1605
8:30 "	661	2103	1666

D. $m = 0.1231$.

Time.	Min.	$K \cdot 10^8$ at 28°	$\frac{K}{m} . 10^8$
Jan. 19. 10 a m.	0	426	3461
10:20 "	20	435	3534
12:20 p. m.	140	446	3623
2:20 "	260	450	3655, Sol. opalescent
4:20 "	380	451	3664, Ppt. forming
7:00 "	540	453	3680
Jan. 30.	11 days.	454	3688 Ppt. all settled down.

GRAPHIC REPRESENTATION OF THE RESULTS.

Fig. 1 shows the change, with time, in specific-conductivity at 28° C. (Hg at $0 = 1$.)

The ordinates represent the specific-conductivity, and the abscissas represent the time in hours; *m*, as before, represents the *gram-equivalent* concentration.

In Fig. 2 the equivalent-conductivity $\left(\frac{K}{m}\right)$ is represented by ordinates and the abscissas represent the time in hours.

The solution $m = 1.623$, as shown in Fig. 1, *conducts better than any other solution studied, although m ranges from 0.1231 to 5.048.*

Fig. 2 shows (1) the familiar fact, that the equivalent-conductivity is always greater as the dilution is increased; (2) that for solutions from $m = 2.524$ to 0.1231, the equivalent-conductivity increases with time, soon reaching a maximum value in the more dilute solutions. It is also true that the rate of the increase in the equivalent-conductivity is more rapid and pronounced in proportion as the solution is diluted, but the curves for the more dilute solutions do not show this from the fact that most of the salt suffers hydrolysis before the first measurement can be made.

In suming up the work, the following facts and inferences may be enunciated:

(1) The dilute solutions of $SnCl_4$ are hydrolyzed more rapidly than those of medium concentration, while the very concentrated solutions come to equilibrium at once.

(2) The extent to which hydrolysis takes place is no doubt regulated by the law of mass action as set forth by Guldberg and Waage, and we are dealing with a state of chemical equilibrium or balanced action which may be represented thus:

$SnCl_4 + 4HOH \geq Sn(OH)_4 + 4HCl$. One cannot measure the extent to which hydrolysis takes place, however, by simply determining the electrical conductivity of the solution, for the problem is a complicated one. The water goes on supplying OH and H ions and HCl is generated. An inventory of the solution as the reaction progresses would show something like the following:

1. Some dissociated $SnCl_4$, giving Sn and Cl ions.
2. Some dissociated HCl giving H and Cl ions.

3. The stannic compound resulting from hydrolysis, not dissociated (say).

4. H_2O, very slightly dissociated, giving H and OH ions.

5. Non-dissociated $SnCl_4$.

6. Non-dissociated HCl (very small amount). Only 1 and 2, or the dissociated $SnCl_4$ and HCl, take any appreciable part in determining the conductivity of the solution.

We have, therefore, two variables in the problem, that is, the $SnCl_4$ is growing less as hydrolysis goes on, and thus more completely dissociated, while the HCl is gradually increasing and hence not so completely dissociated. The ultimate state of things would seem to be this: as the $SnCl_4$ is transformed the specific-conductivity of the solution is decreased (as far as it depends upon the $SnCl_4$) but as the $SnCl_4$ becomes less its dissociation is more complete, and its conductivity therefore increased. These tend to balance each other. As the HCl increases the conductivity of the solution increases; but as the solution becomes more concentrated with reference to HCl, the acid will not be so completely dissociated. Likewise these tend to balance each other.

(3) The *dilute* solutions of $SnCl_4$ hydrolyze with great rapidity; and when equilibrium is reached, they behave in every respect like dilute solutions of HCl, and in fact an equivalent amount of this acid is generated in the process of hydrolysis.

4. The conductivity of solutions of $SnCl_4$ of medium concentration increases slowly with time and *tends* toward a maximum value, namely, *the conductivity of a solution of HCl of the same equivalent strength.*

5. In the very concentrated solutions, the conductivity does not change with time. This does not show that the solution has not undergone any hydrolysis, for it gives a strongly acid reaction.

6. The stannic compound formed by hydrolysis *does not separate out in the more concentrated solutions*, and the hydrolysis of dilute solutions of $SnCl_4$ is practically complete long before the precipitation of the insoluble compound begins. At no time in the reaction does the stannic hydrate seem to take any part in determining the conductivity of the solution.

This compound is no doubt a colloid in dilute solutions, especially when first formed. We may look upon a colloid as a body in pseudo-solution, that is, a step between a true solution and an emulsion. It is known that the diffusion of colloids takes place slowly, the osmotic pressure is small and there is only a slight lowering of the freezing-point or raising of the boiling-point. With these facts and the fact that colloids do not conduct electricity, we conclude that they are not only larger than ordinary molecules, but that they are not dissociated into ions.

7. Attention was called in the first part of this paper to the abnormally large values of the depressions of the freezing-point as found by Loomis. For $m = 0.04$ ($m = 0.01$ according to Loomis) $\frac{\Delta}{m}$ was found to be 12.61. Considering that one molecule of $SnCl_4$ will give 4 HCl molecules (see reaction above) and that the HCl is completely dissociated in *very dilute* solutions, we shall have eight ions for every molecule of $SnCl_4$. These ions would take part in the conduction of the electricity and also act to lower the freezing-point. The maximum value of $\frac{\Delta}{m}$ would therefore be $(1.86 \times 8) = 14.88$. As stated above, Loomis found $\frac{\Delta}{m} = 12.61$ for a solution which he supposed to be 0.04 normal. He prepared this solution from a more concentrated solution made up according to Gerlach's table. According to this table the *gram-equivalent* solution of $SnCl_4$ has a sp. gr. of 1.0486 $\left(\frac{15^\circ}{4^\circ}\right)$. This was the strength of the solution used by Loomis. The writer found the sp. gr. of a gram-equivalent solution to be 1.0496 $\left(\frac{18^\circ}{4^\circ}\right)$, as may be seen in the first part of this paper.

Reducing Loomis' solution as prepared from Gerlach's table to $\frac{18^\circ}{4^\circ}$, it will be seen that our solutions differ by about two points in the third place of decimals. With these facts before us, Loomis prepared a solution with sp. gr. 1.0486 $\left(\frac{15^\circ}{4^\circ}\right)$, and according to my analysis it was found to be 11.5% *weaker* than a gram-equivalent solution. Loomis' value for $\frac{\Delta}{m}$ (12.61) is therefore 11.5% too small. Its corrected value is 14.25.

A 0.04 gram-equivalent solution of HCl is 94.5% dissociated $\left(\frac{\mu_x}{\mu_\infty} = \frac{3400}{3600}\right)$. From this it follows that the theoretical value of $\frac{\Delta}{m}$ is $14.88 \times \frac{94.5}{100} = 14.06$. It is thus seen that this value corresponds well with the corrected value, 14.25, as found by Loomis.

8. Stannic chloride does not furnish an exception to the "dissociation theory"; for when account is taken of its hydrolysis, the phenomena manifested in its aqueous solution in connection with the electrical conductivity and the depression of the freezing-point are explained in accordance with this theory. On the other hand, however, it furnishes direct evidence against the "hydrate theory," for *the hydrate formed takes no appreciable part in the conduction of electricity.* Moreover, it is not probable that water sets HCl free, and then unites with the acid to form a second hydrate. We must still look upon the theory of electrolytes as founded by Arrhenius and others on van't Hoff's view of the nature of solution, as the one nearest in accord with the experimental data.

Finally, I wish to express my gratitude to Professor Loomis whose kindness and assistance have made it possible to carry out this investigation.

PHYSICAL LABORATORY, PRINCETON UNIVERSITY, March, 1899.

NOTES.

Gustav Wiedemann, whose death in Leipzig on March 24, 1899, has recently been announced, was born October 2, 1826, in Berlin. He was educated in the schools of his native city and later at the University of Berlin, where he became a pupil of Rose in chemistry, of Dirichlet in mathematics, and of Magnus, Dove and Mitscherlich in Physics. There were no university laboratories for physics in those days, but Wiedemann with a few other favored students, among whom was Helmholtz,[1] was fortunate enough to be admitted to the private laboratory of Magnus. In 1847 he received the degree of Doctor of Philosophy and in 1851 was enrolled as a Privatdocent in Berlin. In the latter year he married Clara Mitscherlich, a daughter of the physicist.

Wiedemann served for nine years as professor of physics at the University of Basle (1854 to 1863); he then taught the science for three years in the Polytechnic School at Brunswick, and in 1866 became successor to Eisenlohr in the Polytechnic Institute at Karlsruhe. In 1871 he was appointed professor of physical chemistry at Leipzig. In 1877 Hankel, who had been for many years director of the Physical Institute of that University, was retired on account of old age and Wiedemann became professor of physics and director of the Institute. This chair he occupied until his death.

Wiedemann is best known, perhaps, as the author of the compendious treatise on electricity which appeared in two volumes under the title *Die Lehre vom Galvanismus* (Brunswick, 1860 to 1863). It was subsequently rewritten and greatly enlarged as the growth of the subject of electricity demanded and was re-issued under a new name (*Die Lehre von der Elektricität*), in 1882. Still another edition, further revised and largely rewritten with the coöperation of the author's son, Professor E. Wiedemann of Erlangen, was in progress, and two volumes had appeared, at the time of the father's death.

When Professor Poggendorff, who for fifty-three years had conducted the *Annalen der Physik und Chemie*, died, the editorship of this famous periodical was placed in the hands of Wiedemann, and he remained its editor up to the time of his death, during which period sixty-seven volumes were issued. In addition to his labors as editor of the *Annalen* and as author of the treatise on Electricity, a life work in itself, Wiedemann carried on numerous investigations in experimental physics, and particularly in the domain of magnetism. His determination of the conductivity of metals carried on in colaboration with Franz has become classical. It was in these experiments that electrical methods were first used for the determination of the distribution of temperatures. As a lecturer upon the elements of physics and of chemistry Wiedemann was noted for the clearness and simplicity of his exposition and for a delightful fluency and ease of diction. He had personal acquaintanceship with many of the famous experimenters of the first half of the century, and the inexhaustive fund of anecdote concerning them and their work was a delightful and, historically speaking, valuable element of his lectures. He treated physics from the point of view of the chemist rather than of the mathematician, and chemistry very largely from the standpoint of experimental physics.

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[1] See Helmholtz's tribute to Wiedemann in Vol. 50 of the Annalen (1893).

Although he was for many years professor of physical chemistry in the University of Leipzig he did not deal with that subject as it is now treated. The science in its modern sense had scarcely been developed. He was director of two laboratories in the old University Quadrangle on the Augustus Platz in Leipzig; a chemical laboratory and a laboratory of physics. These were always held distinct, although the courses given in them overlapped to a great extent. The work done in both of them, like that of Bunsen in Heidelberg, covered much the same ground as the physical chemistry of the present day, but while it was not physical chemistry in the modern sense of the word it was, at least, useful in that it helped to make the newer science possible. E. L. N.

Wilhelm Gottlieb Hankel, who died in Leipzig, February 18, 1899, was professor of physics at the University of Leipzig for fifty years. Hankel was born May 17, 1814, at Ermsleben, Germany. He studied at the University of Halle where he received the degree of Doctor of Philosophy in 1839. For ten years, subsequently, he was a teacher in the *Realschule* of that town and professor in its University. In 1849 he was called to the chair of physics at the University of Leipzig. The period of Hankel's activity as a man of science lies chiefly between 1839, when he published as his thesis on the *Thermo-electricity of Crystals*, a field in which he was to become an authority, and 1870. His memoirs covered a wide range, but he is best known for his work upon the Electrical Properties of Crystals. He continued to lecture at Leipzig until 1887 and to publish occasional memoirs in the *Annalen* and in the Transactions of the Saxon Academy of Sciences. In that year he was retired from active service on account of old age and was succeeded by Wiedemann. These two men of science, Hankel and Wiedemann, whose loss we deplore, died within a few weeks' time of one another. Experimental physics at Leipzig has been in their hands for half a century and for forty years of this long period Hankel was the head of the *Physical Institute* of that university.

E. L. N.

NEW BOOKS.

Matter, Energy, Force and Work, a Plain Presentation of Fundamental Physical Concepts and of the Vortex-atom and other Theories. By SILAS W. HOLMAN. 12mo. Pp. xiv + 257. New York, The Macmillan Co., 1898.

This book is more than its title would indicate, being an attempt to re-formulate the fundamental physical concepts so as to bring them more into accord with the prominence of the conception of energy in physical science and with the advances recently made in the theory of the constitution of bodies.

As fundamental concepts are those in terms of which fundamental hypotheses or axioms are expressed, the re-formulation of concepts should involve a similar treatment of axioms. That this is desirable, the author does not seem to recognize; for while the concepts are treated with great elaboration, no systematic attempt is made to formulate the appropriate axioms. The reader, therefore, requires to find out for himself what changes in axioms the new concepts would render necessary.

After a short chapter on motion, which is conservative, indeed retrogressive, in character, the concept of energy is introduced. It is defined as "power to change the state of motion of a body," the following, among other comments being made: (1) "Energy stands to change of state of motion as cause to effect," and (2) "Whenever change of state of motion is in progress, there energy must be in action." The author holds, and considers it of importance, that according to his definition it is energy only that can properly be said to produce change of motion; but while this position is consistent with (1), it seems to be inconsistent with the definition itself, since mere power to change can hardly be said to produce change, and with (2) also, which would make energy *in action* the essential condition.

Energy being thus defined anew, the author devotes a chapter to showing that all forms of energy in the old sense will also be forms of what we may call Holman-energy.

Force is now introduced as "that action" ("process, mode of acting," etc.) "of energy by which it produces tendency to change in state of motion of bodies," the context showing that "tendency to change" includes its actual occurrence. Force is thus one or other of two processes, which are apparently assumed to be the same, and according to the author (who considers this also to be important), it (1) cannot be said to be an "entity" in the same sense as energy can, (2) cannot be said to act, though energy can, and (3) can be produced by nothing but energy. But he does not show how mere power can with any propriety be said either to be an "entity" or to act, or how a mode of acting can be said to be produced by a power to change motion.

The quantitative aspect of energy is now reached; and here the author meets with a difficulty inherent in his mode of definition. The practice of defining quantitatively he holds to be "inadmissible in a fundamental treatment," "however harmless in mathematical physics." As his definition of energy is accordingly entirely qualitative, nothing quantitative is to be got out of it. He gets over the difficulty by means of the following "axiom:" "To accelerate equally from rest the free motion of a constant body at different times, equal quantities of energy must be imparted to the body." This, however, is not an axiom merely, for the term, quantity of energy, not having been defined, the statement, as a generalization of experience, is without meaning. It is an axiom in part, for it assumes that under the given conditions equal somethings are imparted to the body; and it is a definition in part, for it tells us what the term equal-quantities-of-energy is to mean. Armed with this axiom-definition the author appeals formally to experience, imagining experiments made with a machine by the aid of which moving bodies can exhaust their motion-changing power in the production of acceleration in a standard body; and (to sum up his results) he finds (1) that if different bodies with different velocities produce the same acceleration in the standard body, and thus afford evidence of the existence of equal quantities of energy, it is possible to attach to the different bodies numerical values K_1, K_2, etc., such that if V_1, V_2, etc. are the velocities of the bodies, $K_1V_1^2=K_2V_2^2=$etc., and (2) that the K's are the same, whatever the time of the observations and whatever the acceleration produced in the standard body may be. These constants K, he calls the kinergeties of the bodies. He then assumes that the kinergety of a body is proportional to the quantity of its kinetic energy when it has a given velocity, *i. e.*, he defines quantity-of-kinetic-energy in terms of kinergety. Thus he obtains two equations: $K \propto 1/V^2$ when H (*i. e.*, Holman kinetic energy) is constant, and $K \propto H$ when V is constant; whence $H \propto KV^2$.

As kinergety and mass (in the ordinary sense) are both quantities which have at all times the same value for the same body, they are proportional to one another, as also consequently are kinetic Holman-energy and kinetic energy. The author assumes throughout the book that kinergety and mass are proportional; but as his "mass" "is nothing more than a verbal substitute for the words quantity of matter," the assumption of their proportionality is quite gratuitous, if it has any meaning at all.

We are next introduced to the phenomena of transference and transformation of energy and the law of conservation to which they are subject. The law, as formally stated, is the law of conservation during transference and transformation only, and does not assert conservation during residence in a body or medium; but the context shows that this restriction is not intentional. Sketches are given of the historical development of this generalization and of the experimental evidence on which it is based, and some instances of transformation are considered. It should be noted that the law of conservation as historically developed, applies to energy as ordinarily defined, not to Holman-energy. And indeed as quantity of non-kinetic Holman-energy has not yet been defined, a law of conservation of Holman-energy, if stated, would have no meaning. The author, however, evidently intends to assert the conservation of Holman-energy as a fundamental axiom. For him, therefore, the law is at this stage, in part a definition, quantity of Holman-energy of any form being tacitly defined as equal to the quantity of kinetic energy into which it is transformable, and in part hypothesis, the quantity of kinetic energy into which it is transformable being assumed to be independent of time and mode of transformation.

The specification of force presents a difficulty; for as force is a mere process or mode of acting, one does not see why it should have direction or place of application or magnitude. It is assumed, however, to have all three; and it is asserted (1) that the direction of the mode of acting is the direction in which the body subjected to it would move if free, (2) that the point of application is the point at which the mode of acting is applied, and (3) that the magnitude is "naturally measured by the rate [*i. e.*, space rate] at which energy is imparted." The critic naturally holds that the direction, point of application, and magnitude of force have been defined quite independently of the definition of force itself. It follows, by the aid of these supplementary definitions, that Holman-force is proportional to the product of kinergety into acceleration, and therefore to the product of mass into acceleration, and therefore to ordinary force.

Work is defined as "any process of transference or transformation of energy," and like force is thus relegated to the limbo of non-entities. It is a process, "as force is, but of a different nature." If the body acted upon has sufficient freedom of motion, "the energy will both exert force and perform work." Thus in such a case energy (power to change motion) produces two processes of different nature. This view certainly does not conduce to clearness; but it admits of subtle language distinctions, as, *e. g.*, that it is permissible to say: work done under or against a force, but not to say: work done by or under the action of a force. One wonders how work, a process, can be said to be done at all, and if it can, why the other process, force, cannot be said to be done also.

The context contains a curious modification of the definition of work, viz., that the word transference is employed in it in the sense of imparting from one portion of substance to an adjacent one. The author apparently regards the conception of work as not applicable in distance-action treatments of physical problems except in cases in which transformation of energy only is involved. But there is nothing in his concepts to prevent their being employed both in distance-action and in contact-action modes of treatment.

It is asserted to be "evident from the nature and definition of work that it is to be measured by the quantity of energy transferred or transformed," and therefore by the product of Holman-force, and consequently of ordinary force, into co-directional displacement. As we know nothing of the nature of work except from the definition, this evident-ness cannot be admitted. Thus in reality a quantitative definition has again been superposed upon the original qualitative one.

Finally the term potential energy is used to denote energy which either is not, or is not known to be, kinetic, and its treatment will thus be obvious.

The closing chapters of the book contain sketches of the kinetic theory of gases, Le Sage's theory of gravitation, and the vortex-atom theory of the constitution of bodies. In the light especially of the vortex-atom theory, the author is led to distinguish between "primary substance," devoid of energy, to which he would restrict the term matter, and "substance," specifically so called, resulting from the association of energy with matter. The properties of substance are the properties of bodies, which are bounded portions of substance. The properties of "matter" he endeavors to get at by a process of exhaustion ; but as might be expected, he is able to reach no definite conclusion ; for the properties of the primary substance are to be determined not by ratiocination, but by investigation. If we must ascribe properties to it at all, at present, they must be considered to be the properties of a perfect fluid, these being the properties that the vortex-atom theorist finds he must ascribe to it in order to account for physical laws.

"Matter" is asserted to be quantitative ; and the term mass is proposed as a synonym for quantity of matter, quite regardless of two facts (1) that this term is already almost universally employed in a quite different sense, and (2) that as yet there is no concept, quantity of matter, to require a name. This second objection is met by assuming (*i. e.*, defining) quantity of matter as proportional to quantity of substance, itself as yet a mere phrase. Quantity of substance is next assumed to be (*i. e.*, defined as) proportional to kinergety. Hence quantity-of-matter becomes proportional to kinergety also, and both it and quantity-of-substance, proportional to weight at one place. The quantities-of-substance of two bodies may thus be compared by means of the balance; and it is proposed that when so determined they shall be called the weightals of the bodies.

This conception of weightal the author hopes will bring to an end the controversy between chemists and physicists as to the use of the terms weight, mass, atomic weight, atomic mass, etc. Neither mode of expression, according to the author, is justifiable, weightal, atomic weightal, etc., being the only defensible terms; and he holds this on the ground that what the chemist means by weight is quantity-of-substance. The chemist, however, knows nothing of "quantity-of-substance." He has found it possible to account for phenomena by assuming that his elements enter into combination in the form of little bodies which have a definite relation to one another with respect to their weights (combination must occur at one place), and have, therefore, the same relation with respect to their masses. Obviously, therefore, atomic weight and atomic mass are equally appropriate terms, even if the term weight be restricted to its sense of force. Atomic weightal would be justifiable also; but if the chemist and physicist quarrel about the two terms they have, what may they not be expected to do if given a third!

So far then as the writer is able to judge the author cannot be said to have been successful in re-formulating the physical concepts. He makes no use of his formal definitions except to draw from them precepts, which in some cases refuse to be drawn, as to what we should say and refrain from saying; and as these definitions form really no part of his system, which would stand without them, even those precepts which are deducible from them are not binding. Though the independent quantitative definitions, with the axioms expressed in terms of them, form a complete statement of the foundations of dynamics (the axioms employed being the constancy of kinergety and the conservation of energy, which in hypothetical content, are equivalent to the axioms ordinarily employed), the definitions and axioms are so inextricably intermingled that the axioms require to be enunciated piece-meal. The equations by which the axioms are expressed are practically identical with those derived from the axioms ordinarily employed; and there is thus no gain of facility in the discussion of physical problems. While, therefore, the proposed system of dynamical doctrine has no points of superiority over the old system, it has points of distinct inferiority.

Nevertheless the book as a whole is one from which the reader who is acquainted with the current dynamics will derive considerable benefit. For apart from the stimulus to be gained from looking at things from a new point of view, he will find scattered through the work interesting discussions of such topics as resistance to acceleration, the mechanism of the transference of energy, the possibility of all energy being ultimately kinetic in form, and many others. The sketches of the theories of the constitution of bodies, especially of the vortex-atom theory are admirable pieces of exposition; and the critical discussions of these theories, as well as their application to the discussion of such subjects as the nature of chemical energy, are characterized by insight and originality and are worthy of careful consideration. In fact all parts of the book in which the author discusses physical questions are of value. It is only when he endeavors to lay foundations which shall be more fundamental than those of mathematical physics, and must therefore be laid in the metaphysical region, that as a writer on physics he lays himself open to seriously adverse criticism. J. G. MacGregor.

Harper's Scientific Memoirs. Vol. III. Röntgen Rays. Memoirs by Röntgen, Stokes and J. J. Thomson; translated and edited by George F. Barker, LL.D., pp. 76. Vol. IV. Modern Theory of Solution. Memoirs by Pfeffer, van't Hoff, Arrhenius and Raoult; edited by Harry C. Jones, Ph.D. Pp. 134. New York, Harper & Brothers. 1899.

In Professor Barker volume, translations of the original papers of Röntgen are appropriately followed by Stokes's suggestive lecture on the nature of the phenomena. The value of the volume would have been further enhanced, perhaps, if a larger amount of space had been allotted to the valuable work of J. J. Thomson in this field.

The memoirs selected for translation in Professor Jones's volume are well chosen. They give the reader who has not followed the newer work in physical chemistry a satisfactory glimpse of the modern theory of solution.

E. L. N.

Volume IX. *August, 1899.* *Number 2.*

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THE
PHYSICAL REVIEW.

THE SPECIFIC HEAT OF SOLUTIONS WHICH ARE NOT ELECTROLYTES.

BY WILLIAM FRANCIS MAGIE.

1. The specific heat of solutions has already been made the subject of extended researches. In one of these, by Schüller,[1] it was found that with mixtures of chloroform, benzene and bisulphide of carbon the heat capacity was equal to the sum of the heat capacities of the two liquids mixed. In the case of alcohol mixed with these liquids or with water no such simple relation held, but the observed heat capacity was higher than that given by this rule. As a part of his famous thermochemical investigations, Thomsen[2] studied the specific heats of about thirty aqueous solutions of acids and salts. Besides giving the observed specific heat, Thomsen calculated the apparent molecular heat of the dissolved substance or solute by subtracting the heat capacity of the water in the solution from the total heat capacity and determining from this difference the apparent heat capacity of a mass of the solute which was equal to its molecular weight. This apparent molecular heat diminished as the dilution was increased, even in many cases becoming negative. This result was confirmed by Marignac,[3] who found besides that with aqueous solutions of sugar and with solutions of sulphur, phosphorus and iodine in bisulphide of carbon, the molecular heats were constants

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[1] Pogg. Ann. Ergänzungsband, V., p. 116, 1871.

[2] Pogg. Ann., 142, p. 337, 1871.

[3] Ann. de Chem. et de Phys., 4 Ser., XXII., p. 385, 1871.

for every concentration. As will be seen from the following discussion, the reason for the differences observed in these solutions lies in their different behavior with respect to osmotic pressure.

2. In this discussion we shall use the method applied by van't Hoff to the study of the molecular depression of the freezing point. The solution considered is supposed to be made up by dissolving n gram-molecules of the solute in N gram-molecules of the solvent (a gram-molecule is a weight of the substance in grams equal to its molecular weight). Let the solution, the mass of which is W, be enclosed in a vessel furnished with a semipermeable wall, and so arranged that pressure may be exerted upon it by means of a piston. By the aid of this vessel and the proper accessory reservoirs of heat the following reversible operations may be performed.

By exerting the pressure p_0 upon the solution we may force out some of the pure solvent through the semipermeable membrane. The diminution in volume of the solute will entail a development of heat, and, to keep the temperature constant at the original temperature t_0, this heat must be abstracted. Let us designate by $\mu_0 dv$ the heat which must be abstracted to keep the temperature constant, while the volume is diminished by dv. Then μ_0 may be called the latent heat of expansion. The compression is continued until a mass w of the solvent is removed from the solution. The work done by the external pressure is $\int_{v''}^{v'} p_0 dv$ and the heat abstracted is $\int_{v''}^{v'} \mu_0 dv$, when v' and v'' are the volumes of the original solution and of the more concentrated solution after the mass w of the solvent is removed.

We then cool the solution and that portion of the solvent which is outside the vessel to the slightly lower temperature t. To do this we remove the quantities of heat $s''(W - w)(t_0 - t)$ and $sw(t_0 - t)$ from the solution and the solvent respectively. The symbols s'' and s represent the specific heats of the solution and the solvent respectively.

We then allow the solvent to enter the vessel again through the membrane. By this operation work is done against the external pressure p, which may be different from p_0 because of the difference of temperature, and heat is consumed, which must be supplied from without to keep the temperature constant. Using similar symbols to those already introduced, we shall represent the work done by $\int_{v''}^{v'} p\,dv$, and the heat supplied by $\int_{v''}^{v'} \mu\,dv$. In this statement we ignore the slight change of volume of the liquids due to the difference of temperature.

We then raise the temperature of the solution, which now contains all the solvent, to the original value t_0. To do this we introduce the quantity of heat $s'W(t_0 - t)$ where s' is the specific heat of the original solution.

By these operations, which are all reversible with respect to the liquids serving as the working substance, we have brought the system back to its original state. We may, therefore, write the equations of energy and entropy for this cycle. To form the first of these we set the heat removed from the system equal to the work done upon it, and have, on rearranging the terms,

$$(a)\quad \int_{v''}^{v'}(\mu_0 - \mu)dv + \{s''(W - w) + sw - s'W\}(t_0 - t) = \int_{v''}^{v'}(p_0 - p)dv.$$

The second equation is formed by substituting in the equation $\int \frac{dQ}{T} = 0$ the appropriate values of dQ and of T for each of the four operations. If the temperatures t_0 and t are taken on the absolute scale, this equation becomes

$$(b)\quad \int_{v''}^{v'}\left(\frac{\mu_0}{t_0} - \frac{\mu}{t}\right)dv + \{s''(W - w) + sw - s'W\}\int_{t}^{t_0}\frac{dt}{t} = 0.$$

Now $\int_{t}^{t_0}\frac{dt}{t} = \log\frac{t_0}{t} = \frac{t_0 - t}{t}$, if the difference of temperature is so small that we may neglect all terms in the expansion beyond the first. Hence (b) becomes

$$(c)\quad \int_{v''}^{v'}\left(\frac{\mu_0}{t_0} - \frac{\mu}{t}\right)dv + \{s''(W - w) + sw - s'W\}\frac{t_0 - t}{t} = 0.$$

Now v and t are independent, so that we may introduce t as a factor in the integral with respect to v, and hence from (a) and (c) we get

$$(d) \qquad \int_{v''}^{v'} \mu_0 \left(\frac{1}{t} - \frac{1}{t_0}\right) dv = \int_{v''}^{v'} \left(\frac{p_0 - p}{t}\right) dv.$$

It was shown by van't Hoff, and his conclusion has been repeatedly confirmed, that the osmotic pressure in many solutions is proportional to the absolute temperature. If the pressure exerted in the operations described is the osmotic pressure and if it obeys this law we may set in (d) $\frac{p}{t} = \frac{p_0}{t_0}$, or $\frac{p_0 - p}{t} = p_0 \left(\frac{1}{t} - \frac{1}{t_0}\right)$, and obtain (e) $\mu_0 = p_0$; or, the osmotic pressure equals the latent heat of expansion. This relation is well known and has been frequently assumed as the basis of a demonstration that the osmotic pressure is proportional to the absolute temperature.

Using this in (a) or (b) we obtain

$$(f) \qquad s''(W - w) + sw = s' W,$$

a relation which shows that the heat removed in the second operation is equal to that introduced in the fourth.

3. The relation here found is equivalent to the law that the specific heats of the solvent and of the solute are constant at all concentrations. For, let us call the masses of the solvent and of the solute in the original solution M and m respectively, and let us use σ to designate the specific heat of the solute in the solution. Then by the law just stated we may write for the heat capacity of the solution before and after the mass w of the solvent has been removed

$$(g) \qquad \begin{cases} s'(M + m) = sM + \sigma m \\ s''(M - w + m) = s(M - w) + \sigma m \end{cases}$$

from which we get (f), since $W = M + m$.

4. This law has been deduced from the assumption that the solute exerts a pressure under the following limitations :

1. The pressure exerted by the solute must be proportional to the absolute temperature.

2. The work done against any external pressure, say that of its own vapor, in forcing out the solvent, is neglected.

3. The specific heats are supposed to be independent of the temperature.

4. The change of volume consequent upon change of temperature is neglected.

Of these the first is the only one that has been formally introduced into the equations. According to the investigation of Schiller[1] the second introduces a term which is of the second order of small quantities and is assumed by him to be negligible. There are no data at hand by which we can examine this matter fully, but I hope to be able to consider it at some future time. It may be that this term is involved in the explanation of the behavior of such solutions as those of alcohol in water, in which solution is attended with a considerable change of temperature and diminution of volume. In these cases, however, it seems that the pressure which must be exerted to force out the solvent will not be that merely which is required to compress the solute as a gas, but different from that, in consequence of the molecular interactions between the solute and the solvent, and that this pressure may be a function of the temperature which does not conform to the first condition.

It is one of the fundamental propositions of the modern theory of solutions that non-electrolytes in solution exert an osmotic pressure which is proportional to the absolute temperature, and if these substances do not develop or absorb heat on going into solution it seems probable that the pressure needed to force out the solvent is equal to the osmotic pressure; so that such solutions should exhibit specific heats that conform to the simple law stated above. Electrolytes should also conform to the law when the dissociation is complete. But before dissociation is complete, it would seem that the osmotic pressure of electrolytes should not be exactly proportional to the absolute temperature. Certainly the dissociation of a gas is a function of temperature as well as of volume, and there does not seem to be any essential distinction between the dissociation of a gas and that of an electrolyte in solution. Furthermore, the change of electrical conductivity with temperature is such as to indicate a connection between the dissociation and the temperature.

It is true that the founders of the theory of solutions considered Gay-Lussac's law applicable to electrolytes as well as to non-electrolytes, and cited the measurements of Pfeffer and others in support of this view. All that can be said at present in this connection is that the evidence for this view is incomplete. In comparisons of isotonic solutions with each other it may have happened that the change of dissociation was nearly the same in both solutions, so that they remained isotonic without conforming precisely to Gay-Lussac's law. Most of the direct measurements show a departure from Gay-Lussac's law, which is slight, to be sure, but still perhaps sufficient to account for the variability of the specific heats of electrolytic substances. Moreover, the experiments on osmotic pressure were carried out with solutions which were more dilute, as a rule, than those used in the determinations of specific heats, and the more dilute solutions should conform more closely to Gay-Lussac's law.

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[1] Wied. Ann., Vol. 67, p. 299, 1899.

5. In case the pressure is not proportional to the absolute temperature, we may return to equation (*a*), which holds in any case. If we divide by $t_0 - t$, and remember that $t_0 - t$ may be as small as we please, we have in the limit,

$$\lim. \frac{\mu_0 - \mu}{t_0 - t} = \frac{d\mu}{dt} \lim.; \frac{p_0 - p}{t_0 - t} = \frac{dp}{dt}.$$

We may then write (*a*) in the form

$$(h) \qquad s''(W - w) + sw - s'W = \int_{v''}^{v'} \left\{ \frac{dp}{dt} - \frac{d\mu}{dt} \right\} dv.$$

But from the known thermodynamic relation $\frac{\partial Q}{\partial v_t} = t\frac{\partial p}{\partial t_v}$, in which $\frac{\partial Q}{\partial v_t} = \mu$, we may deduce $\frac{d\mu}{dt} = \frac{dp}{dt} + t\frac{d^2p}{dt^2}$, and substituting in (*h*) we have

$$(j) \qquad s''(W - w) + sw - s'W = -\int_{v''}^{v'} t\frac{d^2p}{dt^2} dv.$$

Now if we know the specific heats at different concentrations we may calculate from our observations

$$(k) \qquad \begin{cases} \zeta' = s'(M + m) - sM \\ \zeta'' = s''(M - w + m) - s(M - w), \end{cases}$$

following the analogy of (*g*), except that ζ' will not be equal to ζ''. In these equations ζ' and ζ'' are the *apparent* heat capacities of the solute, on the hypothesis that the specific heat of the solvent remains constant. They become the apparent molecular heats when m is the mass of one gram-molecule of the solute. From them we get

$$\zeta'' - \zeta' = s''(W - w) + sw - s'W$$

and hence

$$(l) \qquad \zeta' - \zeta'' = \int_{v''}^{v'} t \frac{d^2p}{dt^2}\, dv.$$

6. Now, as may easily be shown, $\zeta' - \zeta''$ is the difference between the heat capacities of the system in its two states. Since it is a function of the volume of the solution, we may write

$$\zeta' - \zeta'' = \int_{v''}^{v'} \frac{dc}{dv}\, dv$$

where c is the heat capacity, and hence, dropping the integral signs,

$$(m) \qquad \frac{dc}{dv} = t\frac{d^2p}{dt^2}.$$

From this it follows, when $p = t\cdot\text{cons.}$, since then $\frac{d^2p}{dt^2} = 0$, that $\frac{dc}{dv} = 0$, which expresses the result in the special case of non-electrolytes.

In this form the results given in the preceding sections were recently published by N. Schiller.[1] They were presented by me to the Princeton Science Club on May 12, 1898, and were published in abstract in the *Princeton University Bulletin* for January, 1899. The delay in publication was due to my desire to confirm the conclusions of theory by experiment. The observations recorded in this paper were begun in November, 1898.

7. I have not been able as yet to do more than examine certain non-electrolytic solutions. In general the results obtained with these confirm the law expressed in equations (g) or (f), § 4. Where they do not do so, as in the case of solutions of which alcohol is a constituent, further study of the properties of the solutions will be needed before the theory, in the form given in equation (l), can be tested. For electrolytic solutions the investigations of

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[1] Wied. Ann., Vol. 67, p. 299, 1899.

Thomsen and Marignac furnish a wealth of material. In these cases also the properties of these solutions must be studied further before the theory can be tested as it applies to them.

8. The method used in the determination of the specific heat was a modification of Pfaundler's method.[1] This method consists essentially in the communication of equal quantities of heat to two similar calorimeter vessels, one containing the standard liquid and the other, the liquid to be observed. The communication of equal quantities of heat is accomplished by passing an electrical current through two coils of equal resistance, one of which is immersed in each liquid. In the experiments to be described this method is especially convenient, for, since the quantity ultimately desired is the molecular heat of the solute, the solvent can be used as the standard, and a suitable adjustment of the weights of the standard and of the solution under examination brings about a nearly equal rise of temperature in both calorimeters. In this case, since the

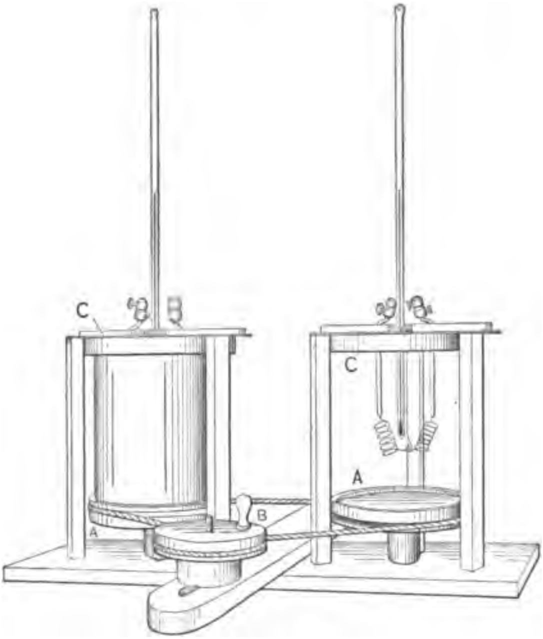

Fig. 1.

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[1] Wien. Acad. Bericht., Vol. LIX., 1869.

calorimeters are similar, the corrections for the calorimeter constants and for radiation become negligible.

The calorimeters (Fig. 1) were cylindrical cups of thin brass, four inches in diameter and six inches high, weighing with the stirrers hereafter to be described, 176 grams each. They were set upon a network of fine cord within larger brass vessels, six inches in diameter and eight inches high, and were centered by three wooden pins projecting from rings that fitted within the exterior vessels. Only one of these vessels is shown in the figure. These exterior vessels, or shields, were set on horizontal wooden disks, *A*, mounted on spindles ast urn-tables and arranged so as to be rotated in the same sense and at the same rate by a cord passing around their circumferences and over a wheel *B*, fixed to an offset from the base. At the bottom of each of the calorimeters was placed a stirrer, Fig. 2, *A*. This consisted of a brass fan or screw made up of four blades radiating from the center and set at an angle of about 60° from the vertical. A part of the lower portion of each blade was cut away so as to prevent the accumulation of liquid between it and the bottom of the vessel. This stirrer was made to fit tight in the calorimeter vessel and was held in position by friction. It was found that when the calorimeters were rotated, the interference of the resistance coils and thermometers with the free motion of the liquids combined with the effect of the stirrers to produce a turbulent motion sufficient to keep the liquids thoroughly mixed. The stirring could be promoted by occasionally changing the sense of the rotation. This method of stirring proved itself highly satisfactory. No errors in the results could be traced to insufficient mixing of the different parts of the liquid.

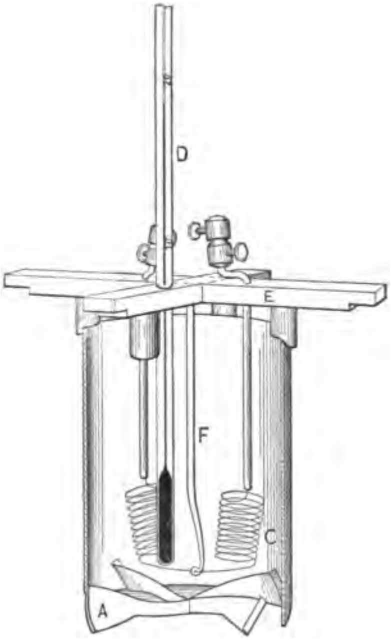

Fig. 2.

The resistance coil, Fig. 2, *C*, and thermometer, *D*, for each calorimeter were carried by a light cross of wood, *E*, the ends of which rested on a ring of tin, Fig. 1, *C*, supported above the vessels by wooden pillars. The coil and thermometer were therefore held fixed, while the calorimeter vessel rotated about them. On the arms of the cross, in suitable positions, were fastened blocks of cork, which cleared the upper edge of the vessel by perhaps an eighth of an inch when it was in position, and which served to support the coils, etc., when the calorimeter was removed from its place for weighing or cooling. It is in this latter condition that the calorimeter is represented in Figure 2. The coil was a spiral of special German silver wire No. 24 B. & S. with a resistance of about 4 ohms and with an exceedingly small temperature coefficient. This wire was very obligingly furnished me by Mr. Edward Weston of the Weston Electrical Instrument Company of Newark, N. J. The ends of the spiral were attached to heavy copper wires, and the middle of the spiral was held down nearly to the bottom of the vessel by a hook made in the end of a glass tube F, drawn out into a thin stem and fixed to the cross. The two parts into which the spiral was thus divided were each of them rigid enough to hold their positions without any appreciable yielding. The copper rods supporting the spiral were fixed near the two extremities of the arm of the cross, and their upper ends were bent in so as to fit into the mercury cups of a Carey Foster Bridge. By means of this bridge the resistances of the two coils could be compared and adjusted whenever required.

The thermometer was borne by the other arm of the cross and was placed about half way between the vessel and its walls. The upper end of its bulb was generally immersed about an inch below the surface of the liquid.

9. The thermometers were made by Green and were carefully compared with a standard whose corrections were furnished by the Reichsanstalt. Their curves of correction showed similar variations in all parts of the scale used, indicating the care with which the tubes were calibrated. The thermometers were graduated on the stem; that used in calorimeter A being divided into tenths, that used in calorimeter B to twentieths of a degree. They were read by means of lenses carried in adjustable clamps, and care was taken

to avoid parallax by bringing the reflection of the mark nearest the end of the mercury column behind the mark itself. They were tapped, before a reading was taken, with a light cork hammer, some trials which were made on a falling column indicating that this was an important precaution. The readings were made by estimation to hundredths of a degree. The coarseness of the marks on the stem rendered this estimation difficult in certain positions of the mercury column, and I do not dare to say that the observations are exact to the hundredths of a degree.

10. In repeating an observation with the same liquids it is necessary to cool the calorimeters to a common initial temperature. To accomplish this most easily and quickly of two rotating turntables were constructed, similar to those already described, but of larger diameter, and furnished with similar rings to support the crosses with their coils and thermometers. On each of these tables was placed a cylindrical copper vessel eight and one-half inches in diameter and six inches high. Within this vessel stood a cylinder of coarse wire netting centered by three projecting arms. Its diameter was such that it easily received the calorimeter vessel. The space between the wire cage and the external walls was packed with snow or broken ice, and it was found advantageous to fill the vessel about one-third full with water. The calorimeters placed in the wire cylinders were quickly cooled to the desired temperature and a little experience made it easy to remove them at such times that their final temperatures did not differ by more than one or two tenths of a degree.

11. The current for the coils was furnished by a dynamo-machine. A contact maker, a Weston ammeter and a variable resistance of coarse German silver wire, with a sliding contact, were included in the circuit. The current remained fairly constant during a set of observations. In different cases it varied from 4.2 to 5.2 ampères.

12. The weighings were performed on a large balance, which was sensitive to 5 mg. with a load of 1 kilogram.

13. The course of a set of observations was as follows:

After it had been determined what was the proper amount of solution to be used to give approximately equal temperature changes in the two calorimeters, the calorimeters, with their coils and thermometers, were placed in turn on the balance and were filled with the solution and the solvent respectively, care being taken that enough liquid was used to well cover the coils and the bulbs of the thermometers. The calorimeters were then placed in the coolers and withdrawn when the desired temperature was reached. If possible this temperature was so determined that the rise to the room temperature was equal to that allowed beyond the room temperature in the observation. This was not always convenient, and in view of the similarity of the two calorimeters and the consequent equality of their radiation coefficients, no great stress was laid on attaining this condition. The calorimeters were wiped dry and placed in their respective positions. If after a few rotations of the turntables it was found that their temperatures differed by more than two-tenths of a degree, this difference was adjusted by warming the cooler one in the hands. The connections with the coils were then made and the vessel rotated for thirty seconds, after which the thermometers were tapped and read as quickly as possible. The rotation was then resumed and after the lapse of one minute from the stoppage of the first rotation, the thermometers were read again. If any irregularity in the rise of the thermometers was observed this process was repeated, but ordinarily these two readings sufficed to fix the initial temperature. The current was then thrown in, and the calorimeters rotated, care being taken to change the sense of rotation every now and then. When the desired elevation of temperature was nearly reached, the current was cut off and the rotation continued for thirty seconds. To render the conditions in this part of the observation as nearly the same as possible for all the observations, the rotation was carried out for ten seconds in one sense, then reversed for ten seconds, then carried out for five seconds in the first sense, and again reversed for five seconds. The thermometers were then tapped and read. After the lapse of one minute, during most of which the rotation was continued, a second reading was made, and a third after the lapse of another minute. These readings were made to test the loss of heat by radiation and evaporation, and to furnish the basis for an estimate of the true final temperature.

The calorimeters were then, if the solvent was, like water, sufficiently volatile to lose weight appreciably during this series of operations, replaced on the balance, and enough of the solvent added to restore the original weights; after which they were cooled, and the observations repeated. In most cases four observations were made with one solution.

14. It was found that water acted upon the brass vessels and the German silver coils sufficiently to conduct as an electrolyte and so to change the resistance of the coils. To avoid this the coils were dipped in melted paraffine, which sheathed them with an insulating coating. No delay in assuming the proper final temperature could be detected on the part of the liquid due to the low heat conductivity of this paraffine coating, and it served its purpose well in preventing changes of resistance in the coils. This precaution was also needed with alcohol, but was unnecessary with aniline. If the resistances of the coils differed by more than a certain amount a correction to the result became necessary. This difference was, however, outside the limits within which the resistances could easily be adjusted, and was rarely, if ever, allowed.

15. The ratio of the specific heats of the liquids in the two calorimeters could be calculated immediately from the formula $\frac{m_2 t_2}{m_1 t_1}$, where m_1 and m_2 represent the masses of the solution and the solvent as standard, t_1 and t_2 the changes of temperature in these masses respectively. The corrections due to the calorimeter constants, to radiation and to the resistances of the coils are all differences of quantities nearly equal for the two similar calorimeters, and may be neglected. The specific heat of the solution was then found by multiplying this ratio by the specific heat of the solvent. From this the specific and the molecular heat of the solute were obtained, by assuming that the specific heat of the solvent in the solution remains unchanged. Since the ratios of the specific heats thus obtained are always very nearly equal to unity, the calculated molecular heats of the solute are not seriously affected by any small error in the assumed specific heat of the solvent. This may, therefore be taken from published tables or results, without requiring a special investigation.

16. The results obtained for the specific heats of the solutions

examined are given in the following tables. The apparent molecular heats of the solutes, given under the symbol ζ, were calculated by assuming that the specific heat of the solvent is unchanged in the solution. The column headed by N gives the number of gram-molecules of the solvent containing one gram-molecule of the solute. The calculated specific heats are those determined from equation (g) by using for σ, the specific heat of the solute, the mean specific heat found from the mean value of ζ.

17. *Cane Sugar in Water.*—The sugar employed was prepared by recrystallization of rock candy. The best crystals were selected, powdered, and carefully dried in a steam bath. The water, as in all cases in which water was used, was distilled in a block tin tube on the plan introduced by Hulett,[1] and was shown, by observations of its electrical conductivity, to be of a high degree of purity. Its molecular weight was taken to be 18.

Cane Sugar ($C_{12}H_{22}O_{11}$) *in Water. Molecular Weight* 342.

N	s' Observed.	s' Calculated.	ζ
50	0.8479	0.8475	153.1
100	0.9115	0.9116	152.5
150	0.9375	0.9377	150.
250	0.9609	0.9609	152.7
Mean molecular heat,			152.6

The mean molecular heat obtained by Marignac for cane sugar was 147.

18. *Dextrose in Water.*—The dextrose was obtained from Kahlbaum. The very slight trace of water which it contained was removed by long drying in a steam bath.

Dextrose ($C_6H_{12}O_6$) *in Water. Molecular Weight*, 180.

N	s' Observed.	s' Calculated.	ζ
100	0.9490	0.9489	79.
200	0.9731	0.9732	78.4
300	0.9819	0.9819	79.
Mean molecular heat,			78.8

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[1] Journal of Chem. Physics, September, 1896.

19. *Glycerine in Water.*—The glycerine was c. p. glycerine obtained from Merck. About equal volumes of this glycerine and of water were mixed and the specific gravity determined by a standardized Mohr's balance. From this the percentage composition of the solution was found by the use of Gerlach's table (Landolt and Börnstein, p. 230), and the solutions used were made up from this stock solution. By a comparison of the weight of water added with that indicated by the specific gravity, the amount of water in 100 grams of the glycerine was determined to be 1.94 grams.

Glycerine ($C_3H_8O_3$) *in Water. Molecular Weight* 92.

N	s' Observed.	s' Calculated.	ς
40.40	0.9556	0.9537	55.6
50.275	0.9621	0.9620	54.2
88.49	0.9771	0.9775	53.4
100.	0.9803	0.9800	54.7
132.96	0.9841	0.9847	52.3
150.	0.9867	0.9864	55.0
250.	0.9917	0.9918	53.9
400.	0.9948	0.9948	54.0
500.	0.9954	0.9958	50.0
Mean molecular heat,			54.1

In taking this mean the result for $N = 500$ was omitted.

20. *Mannite in Water.*—The mannite was obtained from Merck. No pains were taken to dry it or to test its purity.

Mannite ($C_6H_{14}O_6$) *in Water. Molecular Weight* 182.

N	s' Observed.	s' Calculated.	ς
100	0.9662	0.9647	115
200	0.9812	0.9815	113
300	0.9873	0.9875	111
400	0.9900	0.9905	108
Mean molecular heat,			112

These molecular heats show a regular decrease with increasing dilution. I believe that this change is only apparent and not real. In the first place the results for $N = 100, 200, 300$ involve a correction, determined after the last result was obtained, that was assumed to be constant, but may very well have had an increasing value; and in the second place, if the mannite were not perfectly dry, the effect of the water contained in it would be least felt in the more dilute solutions. In my opinion the most probable value for the molecular heat of mannite in water is 108.

21. *Urea in Water.*—The urea was obtained from Merck, in the form of clean, sharp-edged, separate crystals. A similar specimen to that used was dried in a steam bath and showed only an almost inappreciable amount of water present.

Urea (CON_2H_4) *in Water. Molecular Weight* 60.

N	s' Observed.	s' Calculated.	ς
100	0.9796	0.9790	22
200	0.9896	0.9893	22
300	0.9927	0.9929	20
400	0.9945	0.9946	20
Mean molecular heat,			21

22. *Acetamide in Water.*—The acetamide was obtained from Merck. It was found to be dry. When dissolved it cooled the water slightly.

Acetamide $C_2H_3O(NH_2)$ *in Water. Molecular Weight* 59.

N	s' Observed.	s' Calculated.	ς
100	0.9870	0.9868	34.9
200	0.9931	0.9933	34.
Mean molecular heat,			34.5

23. *Ethyl Alcohol in Water.*—The alcohol was Squibb's c. p. alcohol, specially prepared and dried, and was found to be as stated by the maker, 99.89 per cent. pure.

Ethyl Alcohol (C_2H_5OH) *in Water. Molecular Weight* 46.

N	s' Observed.	ς
12.5	1.0509	59.8
50	1.0194	64.3
100	1.0074	59.7
200	1.0026	55.5
300	1.0009	51.2

These observations illustrate the well-known fact observed by Dupré and Page,[1] and by Schüller,[2] that the specific heat of solutions of alcohol in water is greater than that of water. The molecular heats do not show the same constancy as that exhibited by the other solutions which have been presented. They increase with the concentration up to a certain point and then again decrease.

24. *Water in Aniline.*—The aniline used was Kahlbaum's product, obtained from Eimer and Amend. Its color was a yellowish red, which rapidly deepened when it was exposed to the air and heated in the calorimeters. It was at first used as it came from the bottles, but some control experiments were made by mixing the contents of several bottles in a large flask and using the homogeneous substance thus obtained as stock. A quantity, after it had been once used, was recovered by distillation over sodium and used in the same way with similar results. My observations are not in accord with those of Griffiths,[3] who found that the specific heat did not change when the aniline changed color by exposure to the air and to heat. Certain irregularities in my observations which could be explained by supposing that the aniline which I had been using as standard had experienced a change of its specific heat led me to compare it with fresh aniline, with the result that the specific heat of the used aniline was shown to be less than that of the other by more than 0.8 of 1 per cent. In the subsequent experiments the standard aniline was renewed after each set of observations.

The specific heat of aniline was taken as 0.5175, which is the value given by Griffiths (l. c.) for 25°, about the mean between the extreme temperatures in the calorimeters. Its molecular weight, from the formula $C_6H_5(NH_2)$ is 93. In order to make the results comparable with those obtained with water as solvent, it seemed best to make up solutions which should contain about the same number of molecules of the solute in unit volume as in the previous experiments, rather than those in which the gram-molecular concentration should be the same. In general this is attained by making the gram-molecular concentration five times as great as with water.

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[1] Phil. Trans., 1869, Pogg. Ann., Ergänzungsband V., p. 21.

[2] Pogg. Ann., 1871, Ergänzungsband V., p. 116.

[3] Phil. Mag., January-June, 1895, p. 47.

Water (H_2O) *in Aniline. Molecular Weight* 18.

N	s' Observed.	s' Calculated.	ζ
10	0.5425	0.5445	33.0
20	0.5310	0.5312	34.6
25	0.5292	0.5285	36.6
40	0 5242	0.5243	34.4
60	0.5223	0.5221	36.0
Mean molecular heat,			34.9

Of these observations, those for $N = 60$, and $N = 25$ were made with fresh aniline. The solutions for the others were made from the solution for $N = 60$ by adding the requisite quantities of water, and the same standard aniline was used in the observations made on them. In view of what has been said of the change in the specific heat of the standard aniline with use, it seems that special weight should be given to the observations made when the aniline was fresh, or that the other observations are subject to correction. This correction, if due to the change in the standard, would increase the molecular heats determined from them. It seems to me that a more probable value for ζ would be 36. As they stand the numbers serve well enough to support the law which is under discussion in this paper.

It is worthy of remark that the specific heat of water in aniline appears to be nearly or quite equal to 2. The molecular heat is doubled by the solution. It is possible to ascribe this to an increase in the number of degrees of freedom of the water molecule due to its dissociation, and to look on it as additional evidence in favor of the view that the physical molecules of water in its liquid state are aggregates of the chemical molecules. In the present state of our knowledge it is, however, inadmissible to do more than hazard this as a speculation. Until further research has shown how to eliminate the effect of the solvent upon the solute in determining its molecular heat, we cannot consider the molecular heat as dependent upon the condition and behavior of the solute alone. That the solvent does modify the molecular heat of the solute will appear from the observations following.

25. *Glycerine in Aniline.*—The glycerine used contained 1.94

grams of water in 100 grams. Allowance was made for this in the weighing and the specific heat of the aniline used as solvent was calculated as if the water dissolved in the aniline with the molecular heat 36. The specific heat of aniline was taken as 0.5175.

Glycerine ($C_3H_8O_3$) *in Aniline. Molecular Weight* 92.

N	s' Observed.	ζ
12.5	0.5427	76.6
24.6	0.5321	79.7
50.	0.5252	81.
100.	0.5215	83.

There seems to be a decrease of ζ as the concentration increases. In view of this and of the way in which the water contained in the glycerine was reckoned in with the aniline, I have not thought it worth while to calculate the specific heats. One thing is evident from this table, namely, that the apparent molecular heat of glycerine in aniline is not the same as it is in water. The values are about in the ratio of 80 to 54. This difference is far too large to be due to errors of observation.

26. *Urea in Ethyl Alcohol.*—The alcohol used was ordinary alcohol distilled over lime. A large quantity was mixed in a bottle and used as stock. The urea was the same product as that used with water. The specific heat of alcohol was taken as 0.6019.[1]

Urea (CON_2H_4) *in Ethyl Alcohol. Molecular Weight* 60.

N	s' Observed.	s' Calculated.	ζ
42.4	0.5984	0.5979	28.8
70.65	0.5992	0.5994	27.5
Mean molecular heat,			28.1

In this case the molecular heats are constant to within the limits of error of the observations, which were rendered more uncertain than with aqueous solutions by the greater volatility of the alcohol. The molecular heat obtained for urea is different from its value in an aqueous solution in the ratio of 28 to 21. This difference is again so large that I do not feel willing to ascribe it to errors of observation. If ζ were 21, as it seems to be for urea in water, the specific heat of the solution $N = 70.65$ would be 0.5973. The difference between this and the number found, 0.5992, while not large, is outside of the variations of the individual results from which the mean was determined.

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[1] Schüller, Pogg. Ann., Ergänzungsband V., p. 119.

Yet, while this is true, there may be some unnoticed error in the measurements which, if detected, would account for this slight difference; and I do not feel that the question will be settled without more extended investigation.

27. *Glycerine in Ethyl Alcohol.*—The glycerine was the same product as that used before. Allowance was made for its water content by adding the appropriate amount of water to the alcohol used as standard.

Glycerine ($C_3H_8O_3$) *in Ethyl Alcohol. Molecular Weight* 92.

N	s' Observed.	ζ
35.32	0.5967	46.25
70.65	0.5967	37.5
141.3	0.5975	27.0

In this case there is a variation of the molecular heat similar to that exhibited by solutions of alcohol and water.

28. It is evident that the prediction of the theory is fulfilled in very many of the solutions which have been examined. In these cases we may safely say that the conditions upon which the theory was based have also been fulfilled and we may therefore expect the depression of the freezing point to correspond with that indicated by van't Hoff's formula. This has been proved for the aqueous solutions by the recent experiments of Professor Loomis.[1] The solution of alcohol in water as measured by him does not agree precisely with the van't Hoff formula; and this may have something to do with the anomalous behavior of its specific heat. In solutions containing alcohol, either as solute or solvent, it seems as if the law of equation (g) did not hold. The anomalous behavior of alcohol in this respect was proved by Schüller for solutions of alcohol with water,

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[1] Princeton University Bulletin, January, 1899.

benzene, chloroform and bisulphide of carbon, and is proved by my experiments for solutions of glycerine in alcohol, and for dilute solutions of alcohol in water. The one exception seems to be in the case of urea in alcohol. Even in this case it is not proved by my observations that the specific heat of urea in alcohol is constant. The two results obtained, for example, may lie on either side of a maximum, such as is exhibited by alcohol in water.

29. I do not feel that the results before me warrant a definite declaration as to whether or not the molecular heats obtained are independent of the solvent. On the modern theory of solutions it would seem that the specific heats, as well as the other properties of the solutes, should be those which they would exhibit in the gaseous state. The specific heats obtained for cane sugar, dextrose, glycerine, mannite and urea, dissolved in water, are not inconsistent with this view. On the other hand, the value 36 obtained for the molecular heat of water in aniline, speaks decidedly against it. The specific heat of water vapor is about 0.5, so that the heat capacity of water in solution in aniline is about 4 times as great as that of water vapor. So far as this example carries any weight, it shows that the solute is not so entirely independent of the solvent that its energy is unaffected by the solvent. The same conclusion may be drawn from the molecular heats of glycerine in aniline and of urea in alcohol, though the former of these cases may be one similar to that of alcohol in water, as indicated by the variations of the molecular heat with concentration.

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AN INTERFEROMETER STUDY OF RADIATIONS IN A MAGNETIC FIELD. II.

BY JOHN C. SHEDD.

PART TWO. THE INTERFEROMETER METHOD.

Professor Michelson[1] has shown the peculiar adaptability of the interferometer for the class of research here considered.

The instrument used in the present case was received March 1, 1898, and has proved highly satisfactory for the work undertaken.

The instrumental difficulties attending the use of the Interferometer are such as perhaps to warrant a word about them. These difficulties arise from two sources : (1) defective workmanship in the instrument, and (2) those due to the observer. These will be considered in order : (1) the most serious error that can be present is a lack of parallelism in the ways along which the mirror carriage moves. This is generally manifested by a shifting of the system of fringes in the field of view, and a more rapid loss of visibility on the part of the fringes than should take place.[2] A second defect is sometimes present in the optical quality of the glass, due either to a defective surface, or to internal stress in the glass. This defect is manifested by a distortion of the fringes which under proper adjustment should appear as circles. A third defect, easily overlooked, is in the silvering of the surfaces. The two end mirrors (see Fig. 10) M' M'' should be heavily silvered, and brightly polished. The half-silvered surface (M), however, should have such a film as will transmit and reflect equal amounts of light. If this be true,

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[1] Astro. Phys. Jr., 6, p. 48, also 7, p. 131.

[2] Phil. Mag. (5), 34, p. 286.

50 % of the incident light is effective in the two interfering beams, and the fringes are of maximum brightness. There is no direct method of estimating this defect, and experience is the only proper guide. (2) A very considerable amount of patience is necessary to eliminate or minimize personal errors. Fringes are often obtained which appear satisfactory, but from a lack of exact adjustment have a variable focus, are distorted, and rapidly fade out as the movable mirror recedes. If the instrument be in adjustment the fringes appear as concentric circles, and Michelson has shown[1] that if the incident light be parallel then the fringes are at infinity.[2] If, however, the light be not parallel, then the fringes are in front of the interfering surface (*M*, Fig. 10), upon it or behind it, according as $\triangle$ is greater than, equal to, or less than zero, where $\triangle$ is the difference in the paths traversed by the two beams of light. In general it may be said that the incident beam is not parallel, and hence, that the fringes recede as $\triangle$ increases. If a telescope is used to view the fringes, the focus will have to be altered slightly, and must soon be adjusted for parallel light. If, on the other hand, the naked eye is used, there is a tendency to focus the eye upon the mirror, instead of upon the fringes. Hence an observer is apt to lose sight of the fringes altogether, or to view them out of focus unless considerable care is exercised upon this point. The real work of the interferometer consists in an eye estimate of the visibility of the fringes as $\triangle$ is increased. There is a manifest liability to error in these eye estimates, unless a comparison set of fringes of known intensity is available; but the process in such a case is not only tedious[3] but adds to the amount of apparatus to be looked after. On the whole, it is more practicable, and perhaps, as reliable to depend upon the training of the eye that comes from long practice, and in testing the eye from time to time by means of comparison fringes. However, even after a correction[4] curve is obtained and applied, there is room for a considerable margin of personal error, which is, perhaps, the chief drawback to the method as a whole.

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[1] Phil. Mag. (5), 13, p. 239, 1882.

[2] Except when $\triangle = 0$, when the fringes are on the mirror.

[3] Astro. Phys. Jr., 7, p. 133.

[4] Phil. Mag. (5), 34, p. 283. Such a curve is shown in Fig. 14.

Adjusting the Interferometer.[1]

The general disposition of apparatus for the present work is shown in Fig. 10.

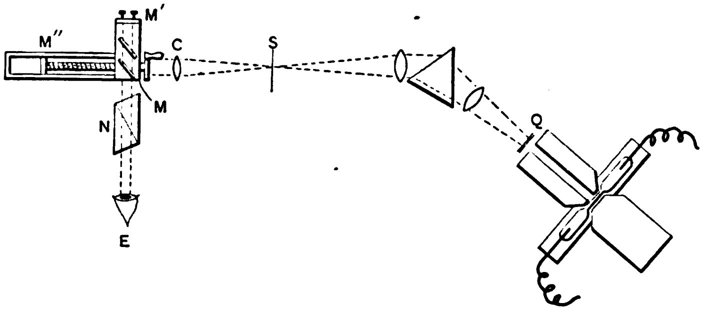

Fig. 10.

M, M', M''.	Interferometer mirrors.
Q.	¼ λ plate.
N.	Nicol.
S.	Slit.
E.	Eye or telescope.

FIRST ADJUSTMENT. *To Find the Fringes.*—The mirror M'' is set so as to be within two or three mm. of the zero of the scale, and a Bunsen flame[2] colored with a piece of sodium glass is so set with reference to the lens C that the field of the mirrors is uniformly bright. Then a pin, or bit of glass fiber, is fastened by a bit of wax onto the lens, so that its image is in the center of the field. It is preferable to have the image lie obliquely across the field, not horizontally or vertically. The mirror M' is then adjusted until the two images of the pin, made by the two mirrors, M' and M'', are superposed. When the images lie obliquely the movements of the adjusting screws can be easily followed. When the adjustment is very close, if the room is partially darkened, and the eye focused for a point behind the mirror, the fringes should appear as fine lines covering the field. The eye should now be focused on these fringes, and the screws adjusted so that the fringes grow broader and more distinct. The final adjustment is reached when they appear as concentric circles, which do not change in appearance, when the eye is shifted in any direction. Under these conditions the two mirrors, M' and M'', are optically parallel to each other.

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[1] The Adjustments of the Interferometer are discussed at length by Professor Wadsworth in PHYS. REV., Vol. 4, p. 480. See also p. 400 of same vol.

[2] The Bunsen flame occupies the position of slit S in Fig. 10, the rest of the system to the right of S being absent.

SECOND ADJUSTMENT. *To Find Zero of the Instrument.*—The zero of the instrument is the position of zero difference of path between the two interfering beams of light. It is found by obtaining fringes with white light and adjusting on the central black fringe of this system. To obtain fringes with white light the mirror M' should first be adjusted so as to give the fringes as vertical lines of small curvature. If now the mirror M'' be moved back and forth by slow motion, the fringes will be seen to change curvature at one region. The exact point of this change of curvature is the point sought. Having adjusted approximately for this, a candle flame should be set before the Bunsen flame, giving the white image of the candle flame with a background of sodium fringes. The mirror M'' may now be slowly moved by means of the tangent screw, the motion being followed by the eye, in the progression of the (faint) sodium fringes. When the zero point is approached brilliant chromatic fringes, from ten to twenty in number, will appear in the image of the candle flame. If now the candle be removed, and the air cut off from the Bunsen flame the chromatic fringes are very distinct and brilliant.

The zero point will shift a little with change of temperature, and still more by a change in the distance of the mirror M' from M due to difference in the manipulation of the adjusting screws. The zero should therefore be observed before each set of observations.

Visibility Curves.

The theory of the visibility curve has been given by Michelson, and its adaptation shown to problems where the distribution of light as a source of radiation is to be determined.

The following curves were taken using sodium light as source of illumination, and viewing the light first perpendicular to the field, and than parallel to it.

I. Perpendicular to the Field. The curves taken are shown in Fig. 11, *A*, and the corresponding distribution of light in Fig. 11, *B*[1].

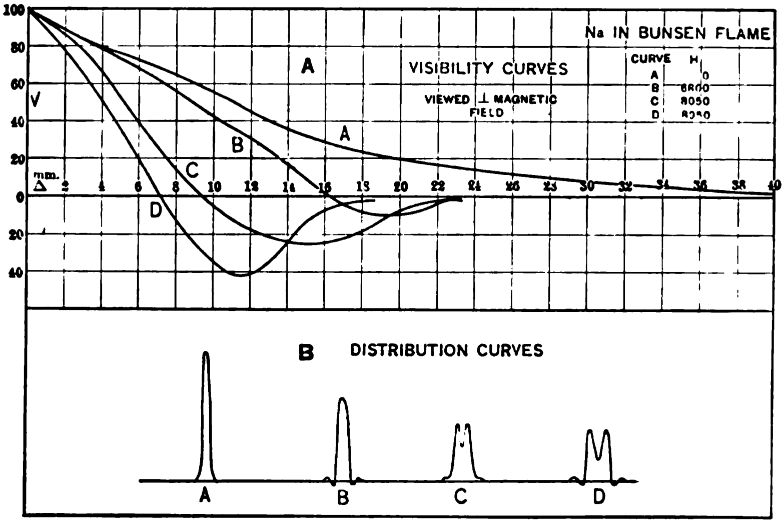

Fig. 11.

In this curve the nicol prism was not used, and the loss of light by the central component (which is known to be present), is such as to render the line an apparent double.[2] In the curves *B*, in those marked *B*, *C*, *D* the main components seem accompanied by small companions some having negative ordinates. Now since this would indicate negative intensities, they indicate errors in observing the curves *A*. The analysis of the curves *A* then furnish a valuable check upon their correctness.

II. Light Viewed Parallel to the Magnetic Field. Curves were taken for this position both with and without the nicol. The results are shown on Figs. 12 and 13. These are substantially identical and show first a broadening and then a doubling of the spectral lines. The presence of the negative ordinates is also seen and yet the substantial results are clear.

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[1] Acknowledgment is due to Professor Michelson who kindly facilitated the analysis of these curves on the Harmonic Analyzer at the University of Chicago.

[2] See part I, also Michelson, Astro. Phys. Jr., 6, p. 49.

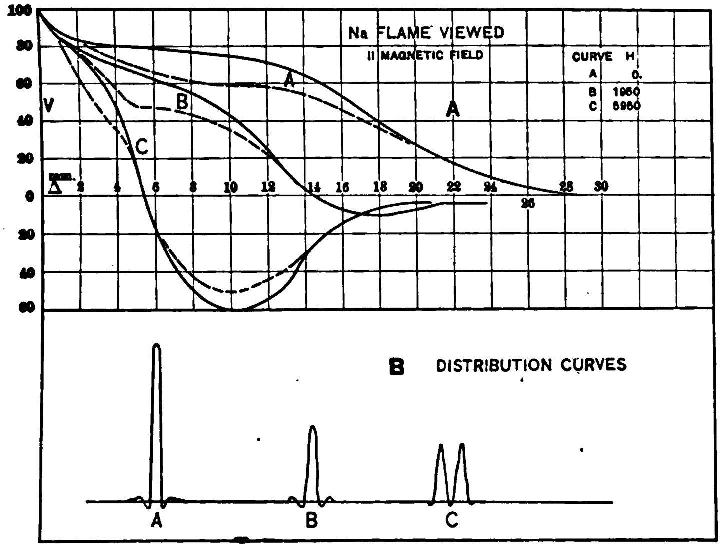
Na FLAME VIEWED
|| MAGNETIC FIELD
CURVE H
A 0.
B 1950
C 5950
100
80
60
40
20
0
20
40
60
mm.
Δ
2 4 6 8 10 12 14 16 18 20 22 24 26 28 30
V
A
B
C
A
B DISTRIBUTION CURVES
A
B
C

Fig. 12.

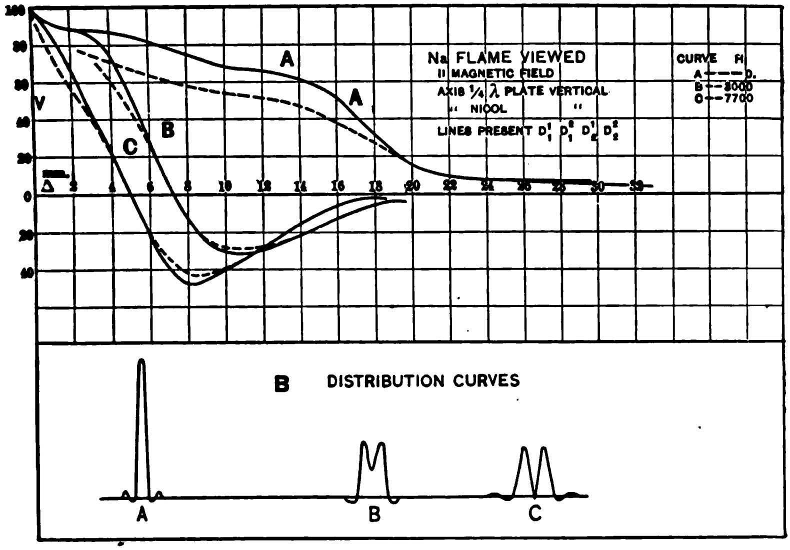
Na FLAME VIEWED
|| MAGNETIC FIELD
AXIS 1/4 λ PLATE VERTICAL
" NICOL "
LINES PRESENT D_1^1 D_1^2 D_2^1 D_2^2
CURVE H
A ——— 0.
B - - 3000
C - - 7700
100
80
60
40
20
0
20
40
mm.
Δ
2 4 6 8 10 12 14 16 18 20 22 24 26 28 30 32
V
A
A
B
C
B DISTRIBUTION CURVES
A
B
C

Fig. 13.

Having analyzed these curves a comparison curve was then taken to observe what correction was necessary to reduce the eye estimates shown in the full line curves of Figs. 11, 12 and 13. The resulting curve is shown in Fig. 14 and the corrected visibility curves are shown in the dotted curves of Figs. 12 and 13. These corrected curves were not analyzed, but their distribution curves would be more nearly correct than those shown. We have then in the curves shown all observational errors present; the result, though not satisfactory, is by no means bad. This form of error is very hard to avoid and long practice alone can eliminate it.

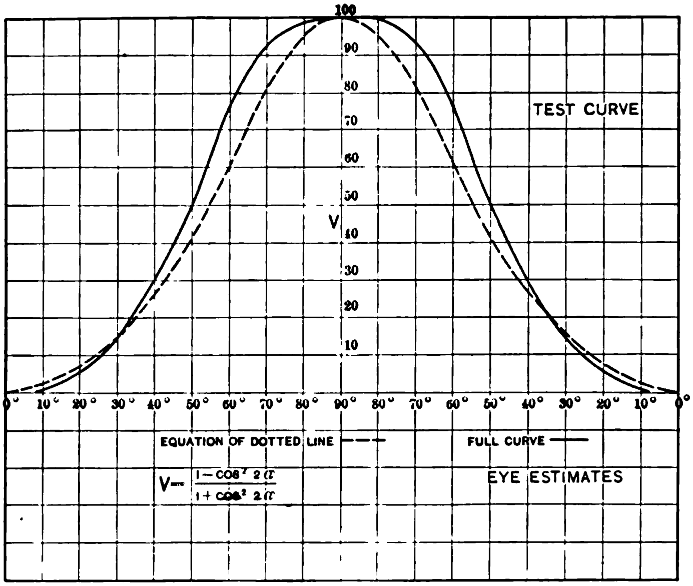

Fig. 14.

Polarization.—The interferometer was found to be capable of showing the state of polarization in the most elegant manner, not only identifying the plane of polarization, but also immediately identifying the accelerated from the retarded ray.

Thus with the axis of the ¼ λ plate vertical the nicol was rotated and the action and character of the fringes observed. The following were the results obtained:

TABLE II.

Position of Axis of Nicol.	Action of Fringes on Magnetization.	Character of Central Fringe with Magnetic Field.
0° Vertical.	Become hazy.	Hazy.
+ 45°	Expand and remain distinct.	Distinct, dark.
+ 90° Horizontal.	Become hazy.	Hazy.
+135°	Contract and remain distinct.	Distinct, light.
180° Vertical.	Become hazy.	Hazy.
—135°	Expand and remain distinct.	Distinct, dark
— 90° Horizontal.	Become hazy.	Hazy.
— 45°	Contract and remain distinct.	Distinct, light.
0° Vertical.	Become hazy.	Hazy.

It is evident that the haziness at 0° ± 90° and 180° is due to the simultaneous contracting and expanding of the fringes. Further analysis shows us that when the fringes expand the retarded component is present and when the fringes contract the accelerated (or shorter wave-length) component is present.

This is shown in Fig. 15. Unpolarized light is represented by the superposition of the two perpendicular planes of vibration.

From this general survey of the interferometer we derive the following generalizations :

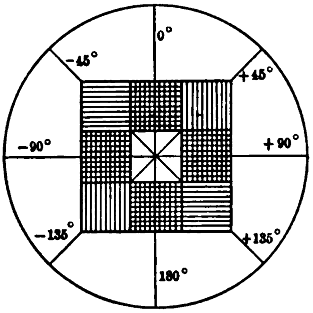

Fig. 15.
Axis of ¼ λ plate vertical,
Nicol rotated.

1. The interferometer is capable of showing the magnetic effect for field strengths below 1,000 C. G. S.

2. The visibility curves, even under unfavorable circumstances, show clearly the general character of the magnetic effect and when checked through a long series, by means of an harmonic analyzer, furnish an incomparable method of analysis.

3. When unaccompanied by such a check errors are not readily eliminated and for quantitative measurements of change in wavelength another use of the interferometer furnishes a better method.

Inasmuch as the visibility curve analysis was, not under the circumstances, entirely available, attention was turned to the development of point 3 above and carried out as follows :

Measurements of Change of Wave-length.

Method.—Professor Michelson[1] in determining the difference in wave-length between the components of the magnetic line makes use of what he calls the "period of coincidence due to doubling." This may be defined as follows: Consider two systems of fringes produced by light sources differing but little in wave-length. Then the *resultant* system due to both sources, will be the resultant of the overlapping of the component systems, and at certain points destructive interference will take place, and at others reinforcement. The fringes will then run through a series of maxima and minima as $\triangle$ is increased. The distance from maximum to maximum is the "period." If $\triangle$ be measured from "zero" to the first point of reinforcement, then the following equation holds

$$\triangle_p = N\lambda = (N + 1)\lambda' \qquad (11)$$

where N is the total number of fringes; and $\triangle$ and λ are measured in mm. In the case of the lines D_1 D_2 these maxima occur every 988 fringes and $\triangle_p = 0.58242$ mm. In the case, however, of the magnetic shift the difference of wave-length is so small that Michelson finds the values of $\triangle$ to range from 58 mm. to 14 mm. as the magnetic field increases to 4,000 C. G. S. units. The same method is also described by Perot and Fabry[2] and is especially applicable where the difference of wave-length is sufficient to give a difference of color to the two sets of fringes.

When applied to the magnetic components the method is open to improvement. In the first place the fringes, at a point where $\triangle =$ 50, or even 30, are so narrow and frequently so faint that an accurate determination of points of reinforcement (*i. e.*, where both systems agree in phase) is not easy. Secondly, since the magnetized and unmagnetized fringes cannot be simultaneously observed, the above determination consists in finding the value of $\triangle$ at which the closing of the magnet shifts the system the double width of one fringe. This determination for high values of $\triangle$ is not as easy as might be wished.

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[1] Astro. Phys. Jr., 6, p. 50.

[2] Astro. Phys. Jr., Feb., 1899. See Appendix III.

There are, however, two possible modifications. If the nicol be not used, it has been seen that the magnetized components draw apart, one being retarded and the other accelerated. The corresponding effect on the fringes is to cause the original system simultaneously to contract and expand. If now the point is found at which the two systems of fringes are tangent to each other, each fringe will have shifted ½ its own width, showing that the accelerated system is ½ period in advance of the retarded one, or ¼ period removed from the original system. We then have the equation

$$\Delta_{\frac{1}{4}p} = N\lambda = (N + \tfrac{1}{4})\lambda'. \tag{12}$$

The second modification consists in using the nicol and in thus quenching one of the component systems. Now concentrate the attention upon one fringe, either adjusting a pointer (fastened to lens *c*, Fig. 10) to its edge, setting the fringe tangent to a line drawn on the mirror *M*, or by merely observing the central fringe of the system. Let it be supposed that the central fringe is dark, and the field unmagnetized. On turning on the current the system contracts, and for a certain value of Δ will be the exact complement of the first, the center being now light instead of dark.

Under these conditions the fringes have shifted over their own width, and the following equation is obtained.

$$\Delta_{\frac{1}{2}p} = N\lambda = (N + \tfrac{1}{2})\lambda'. \tag{13}$$

Still a third modification would be as follows: Let the field remain magnetized and set the fringe, with the nicol in a given position, tangent to the fixed line. Then let the nicol be rotated 90°, so as to bring the other system of fringes into view; then that value of Δ which renders the line a common tangent to both systems gives a difference of period such that

$$\Delta = N\lambda_1 = (N + 1)\lambda_2$$

where λ_1 and λ_2 are the component lines. Also since λ_1 and λ_2 lie symmetrically with respect to λ (the unmagnetized line) we have

$$\Delta = N\lambda_1 = (N + \tfrac{1}{2})\lambda = (N + 1)\lambda_2$$

or, putting the equation in terms of λ,

$$\triangle = N\lambda = (N \pm \tfrac{1}{2})\lambda' \qquad (14)$$

which is identical with equation (13).

Since in the above equations N is the total number of fringes passed over from the position $\triangle = 0$, some means must be had of knowing its value. This is obtained very simply as follows: If λ be given in mm. then $1/\lambda$ equals the number of fringes per mm., and $N = \triangle/\lambda$, $\triangle$ being measured in mm. Substituting this value in the above equations and solving for $\lambda - \lambda'$, the following equations are obtained

$$\left.\begin{array}{ll} \text{For } \triangle_p \text{ equation (11)} & \\ \lambda - \lambda' = \dfrac{\lambda\lambda'}{\triangle} = \dfrac{\lambda^2}{\triangle} & (11') \\ \text{For } \triangle_{\frac{1}{4}p} \text{ equation (12)} & \\ \lambda - \lambda' = \dfrac{\lambda\lambda'}{4\triangle} = \dfrac{\lambda^2}{4\triangle} & (12') \\ \text{For } \triangle_{\frac{1}{2}p} \text{ equation (13)} & \\ \lambda - \lambda' = \dfrac{\lambda\lambda'}{2\triangle} = \dfrac{\lambda^2}{2\triangle} & (13') \end{array}\right\} \qquad (15)$$

For a given, value of $\lambda - \lambda'$ we have the relation

$$\triangle_p = 4_{\frac{1}{4}p} = 2\triangle_{\frac{1}{2}p}. \qquad (16)$$

This last equation would indicate that the second method given by equation (12′) would be the most accurate since it gives the smallest values of $\triangle$ and hence the widest and brightest fringes. It must, however, be noticed that the condition expressed by equation (12) is that the two systems of fringes shall differ by ½ period and hence that the dark rings of one system shall coincide with the light rings of the other. Hence the field will be uniformly illuminated and the fringes disappear. In practice there is found to be a region over which the fringes are blurred and the exact point of extinction is difficult to determine.

On the other hand, in the third method the fringes are always sharp and for values of H above 2,000 C. G. S. units the values of $\triangle$ are small enough to render the fringes sufficiently wide for satisfactory observation.

With a little practice the method of observing the central fringe was found to give concordant readings and hence was used in preference to setting the fringe tangent to a line.

A comparison of the three methods is given by equation (16). Still further light is given by substituting the value of $\frac{\lambda-\lambda'}{\lambda^2}$ from equations (13) in equations (9).

Making this substitution we get

$$\left.\begin{array}{lll} \text{1st method.} & H\triangle = 4\pi v\frac{m}{e}\cdots(11'') \\ \text{2d} \quad `` & H\triangle = \pi v\frac{m}{e}\cdots(12'') \\ \text{3d} \quad `` & H\triangle = 2\pi v\frac{m}{e}\cdots(13'') \end{array}\right\} \qquad (17)$$

If m/e be regarded as constant, these equations all represent rectangular hyperbolæ asymptotic to H and $\triangle$ taken as axes.

It is thus seen that curves expressing the relation of H and $\triangle$ furnish a ready means of comparison both by equations (16), and by their form as indicated by equations (17). Such a set of comparison curves was taken with an oxygen gas flame colored with sodium. In Table III. the values of $H\triangle$ for different parts of the curve are given, and the curves themselves are given in Fig. 16.

TABLE III.

	Method II.		**Method III.**		**Method I.**	
H[1]	Δ	$H\Delta$ (curve A.)	Δ	$H\Delta$ (curve B.)	Δ	$H\Delta$ (curve C.)
2,300	15.4	354	21.4	492	13.1	300
3,000	12.0	360	16.7	500	12.0	360
4,000	9.0	360	13.3	530	11.0	440
5,000	7.3	365	10.5	526	10.0	500
6,000	6.2	370	8.2	490	9.0	540
7,000	5.5	385	6.8	475	8.2	575
8,000	4.8	384	5.8	465	7.4	590
9,000	4.3	386	5.2	470	6.7	602
10,000	3.8	380	4.8	480	6.1	610
11,000	3.5	386	4.5	496	5.6	615
12,000	3.4	408	4.4	528	5.3	635

---

[1] *H* throughout these experiments was determined by the balistic method.

Curves *A* and *B* corresponding to Methods II. and III. show a close correspondence with equation (17) while curve *C* is manifestly unreliable.

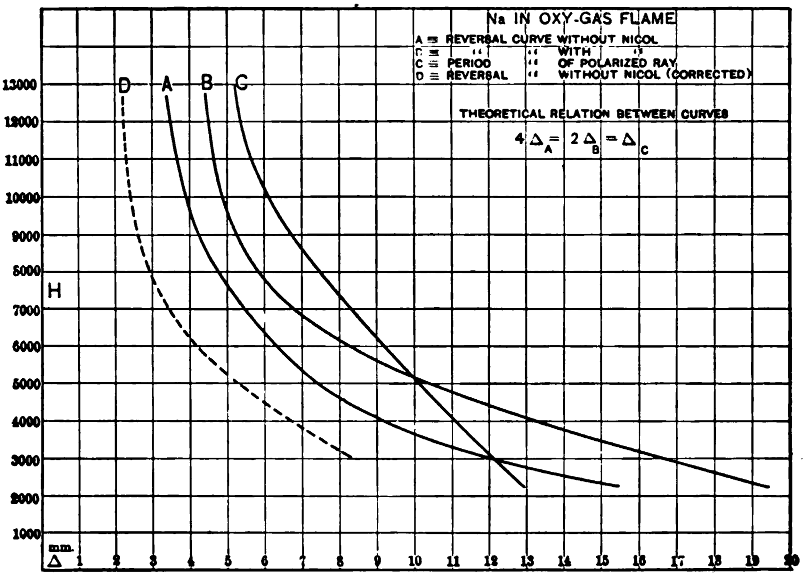

Fig 16.

Comparing curves *A* and *B* we see that equation (16) is not fulfilled, and hence that either one or both curves are displaced. From the fact already pointed out as to the difficulty of making settings for curve *A* we have no hesitation in concluding that the readings for this cure are uniformly too high.[1] Assuming that curve *B* is correct, the dotted curve *D* gives the corresponding position for curve *A*.

SECTION III. COMPARISON OF MAGNETIC SHIFT AT DIFFERENT TEMPERATURES.

To investigate this the sodium flame was used and *reversal* curves taken, by Method III., under the following conditions: I. Bunsen flame, II. Oxy-gas flame, III. Vacuum tube.

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[1] It is not to be supposed that curve *A* represents the greatest accuracy attainable by Method II. Continued observation would undoubtedly render this method quite available.

The light was viewed parallel to the magnetic field as giving the most uniform structure of line (see Fig. 3, *B*), and a $\frac{1}{4}\lambda$ mica plate used. The following tables are selected as representative.

Table IV.

Reversal Curves. Na in Bunsen Flame.

H C. G. S. units.	Δ mm.	λ—λ′ A. U.
0,000	∞	0.000
3,000	12.80	0.135
4,000	11.65	0.155
5,000	10.60	0.168
6,000	9.60	0.180
7,000	8.50	0.205
8,000	7.70	0.230
9,000	6.40	0.274
9,500	5.40	0.325
9,600	5.00	0.350
9,700	4.90	0.370
9,800	4.80	0.375
10,000	4.70	0.380
11,000	4.65	0.380
12,000	4.65	0.380
13,000	4.65	0.380

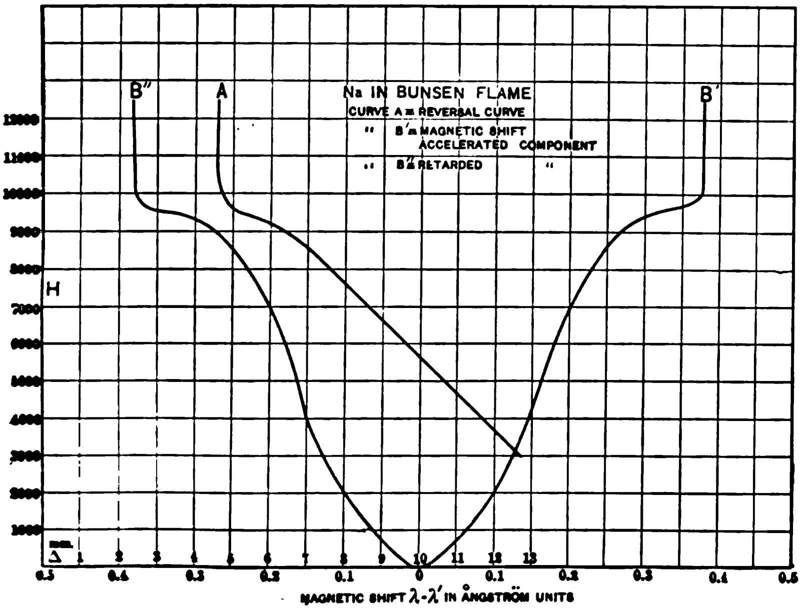

Fig. 17

Table IV. is platted in Fig. 17. The part of the curve lying between 8,000 C. G. S. and 11,000 was checked up with special care and the knee in the curve verified. For all readings where △ is less than 3 mm. a small change in △ indicates a large change in $\lambda - \lambda'$, and hence the readings become less sensitive. The departure of the reversal curve from the normal hyperbolic form would indicate, as is shown in curve *B*, Fig. 17, an ionic inertia or lag, due either to low temperature or high density of the gas, or to both This constraint retards the change in wave-length but suddenly gives way at a field strength of about 9,500. Above a value of *H*, of 10,000 the mean wave-length of the lines do not seem further to change, increase in *H* beyond this point being effective in broadening the lines rather than in separating them.

In curves *A* and *B* (Fig. 18) a small flame 2 cm. high was used with the tip of the inner cone opposite the aperture. In curve *C* the jet was 15 cm. high and the base of the flame opposite the opening. The flame in both cases was colored with a bead of fused sodium carbonate.

Curve *C* shows the same "lag" as does the Bunsen flame, and the maximum value of $\lambda - \lambda'$ is not so large. In curves *A* and *B*,

Table V. See Fig. 18.

Reversal Curves. Na in Oxygen-gas Flame.

H	Δ_A mm.	Δ_B mm.	Δ_C mm.	$(\lambda-\lambda')_A$	$(\lambda-\lambda')_B$	$(\lambda+\lambda')_C$
0	∞	∞	∞	0.000	0.000	0.000
2,500	17.20	18.50	19.40	0.105	0.097	0.090
3,000	16.00	17.15	18.00	0.111	0.105	0.100
4,000	13.30	14.35	15.35	0.135	0.125	0.115
5,000	10.80	11.70	12.80	0.164	0.151	0.140
6,000	8.25	9.00	10.70	0.212	0.197	0.165
7,000	6.20	6.95	9.10	0.285	0.251	0.197
8,000	5.10	5.80	7.80	0.345	0.304	0.228
9,000	4.55	5.10	6.90	0.390	0.345	0.254
9,500	4.40	4.90	6.50	0.400	0.360	0.275
10,000	4.30	4.70	6.00	0.410	0.375	0.310
10,500	4.25	4.50	5.40	0.420	0.402	0.334
11,000	4.15	4.40	5.15	0.430	0.402	0.345
12,000	4.05	4.40	5.10	0.442	0.402	0.345
12,700	4.00	4.40	5.10	0.442	0.402	0.345

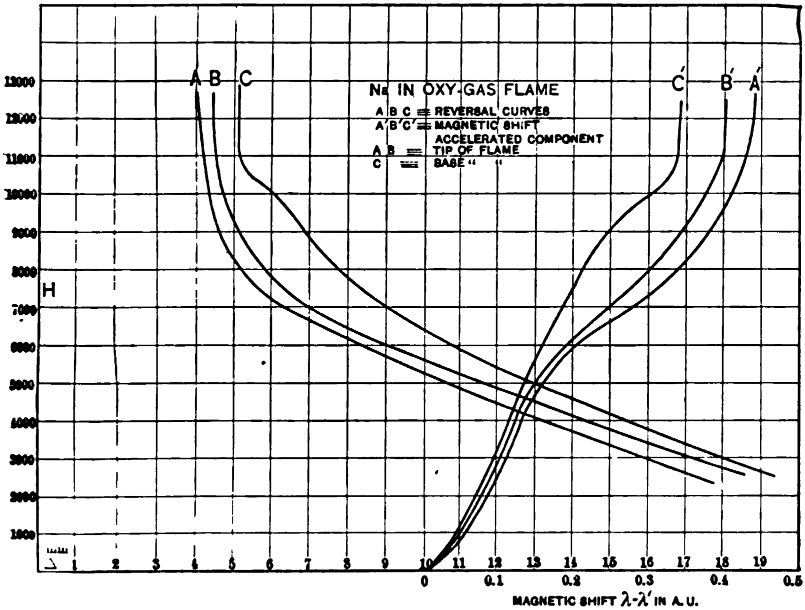

Fig. 18.

which are at a higher temperature, this lag is overcome at a lower value of H, and the maximum value of $\lambda - \lambda'$ is larger.

Table VI. See Fig. 19.

Reversal Curve. Na in Vacuum Tube.

H	Δ mm.	$\lambda - \lambda'$ A. U.
0	∞	0.000
5,000	11.20	0.160
6,000	9.50	0.186
7,000	7.90	0.225
8,000	6.30	0.280
9,000	5.00	0.352
9,500	4.60	0.384
10,000	4.25	0.415
10,500	4.10	0.431
11,000	4.00	0.441
12,000	4.00	0.441
13,000	4.00	0.441

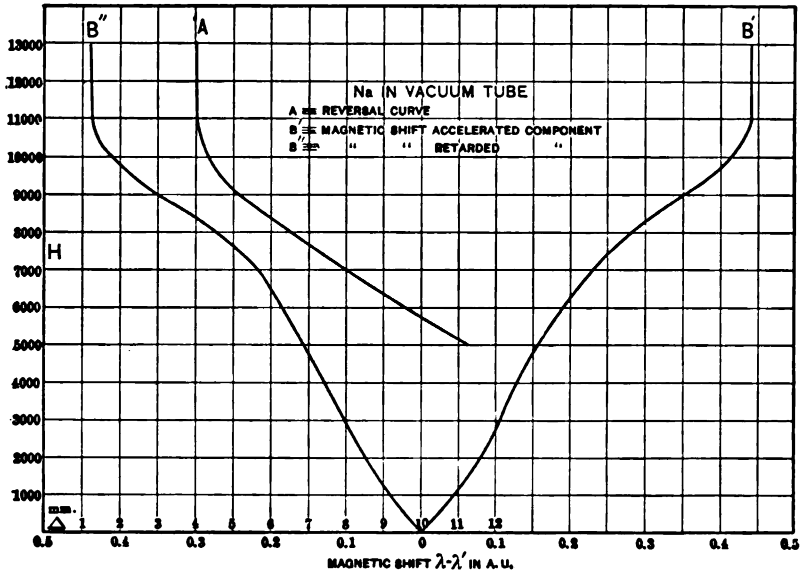

Fig. 19.

Fig. 19 would seem to show that at the temperature of the vacuum tube the change of $\lambda - \lambda'$ is nearly proportional to H up to 7,000 C. G. S., at which point the "lag" is overcome. At 11,000 the maximum for $\lambda - \lambda'$ is reached.

In the case of the vacuum tube, the tube was arranged as shown in Fig. 20. The brass tube T is made to fit over the pole heads of the magnet, and is capped at either end, thus forming a metal box. Into this box the vacuum tube is placed, being held from contact with the metal by asbestos wool.

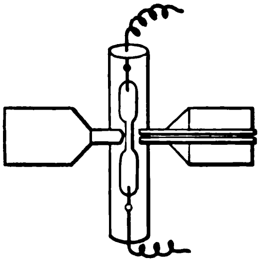

Fig. 20.

A Bunsen flame is so supported as to play horizontally upon the tube. Small glass tubes enter the caps and convey wires to the terminals of the vacuum tube which was energized by an induction coil of the usual type. The substance whose spectrum was to be examined was placed in the tube, in the form of filings, and the tube then exhausted to between 1 mm. and 2 mm. pressure. The brass tube was then heated by means of the

Bunsen flame, until the spectral lines appeared sharp and bright. In some cases the heating was pushed too far, resulting in chemical action on the glass.

Upon using the vacuum tubes it was found that the glass of the tube itself acted as a retardation plate, adding its effect to that of the ¼ λ mica plate. It was thus found impossible to produce an exact retardation of ¼ period. With reversed positions of the nicol prism, however, the two systems of fringes were found to be clear and sharp so that the arrangement was satisfactory if not ideal. The mica plate was used part of the time, and part of the time discarded.

In the preceding curves the effect of pressure cannot be separated from the temperature effect. The combined effect is what is observed. An interesting detail yet to be studied is the case where the pressure is gradually varied, and a pressure curve obtained.

Summary.—From a study of these curves we draw the following conclusions with regard to the effect of temperature.

I. At the temperature of the Bunsen flame there is a distinct ionic "lag" or constraint which is overcome, suddenly, at a field strength of 9,500 C. G. S. units.

II. This ionic lag becomes less as the temperature rises and is practically absent at the hottest temperature of the oxygen-gas flame or of the vacuum tube.

III. The change in wave-length reaches a maximum value depending upon the temperature (and pressure); the maximum is reached at about 11,000 C. G. S. For values of H above this the magnetic effect is to broaden the lines and not to separate them further.[1]

This latter point receives support from Michelson's echelon spectroscope which shows the sodium lines to belong to type II. (See Fig. 3, *B*.

NOTE. An attempt was made to take observations with the spark gap. A glass tube 1 mm. bore was taken, and metallic sodium crowded into it until 2 or 3 cm. of the tube at the center was filled. The tube was then cut at the middle of the sodium, and the free ends used as spark terminals.

On turning on the magnet the electro-dynamic action of the field caused a spark to fan out showing in a very pretty manner the magnetic equipotential surfaces. This has also been observed by M. Cornu. (Astro. Phys. Jr., 7, p. 166, note 2.)

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[1] This may be peculiar to the sodium lines, and be due to the simultaneous presence of the lines D_1 and D_2. No observations have yet been made upon them separately, the dispersion necessary to isolate them being too great.

SECTION IV. MEASUREMENTS OF MAGNETIC SHIFT.

Equation (9) may be put into the form

$$\lambda - \lambda' = \left(e/m \frac{\lambda^2}{4\pi v}\right) H \tag{18}$$

showing that for a given value of λ the change in wave-length should be proportional to H provided no constraint is present. We have seen that in the case of sodium (and presumably for all substances) a constraint is present at low temperatures, but that it disappears as the temperature rises. In the measurement of magnetic shift it is important that the temperature be high, and, preferably, that the pressure be low. These conditions are fulfilled by the vacuum tube; hence it was used as a source of illumination.

It was primarily intended to make an extended series of observations upon the spectral lines of different substances, but the difficulties encountered of breaking tubes, deposits in the capillary, and chemical action within the tube, were such as to reduce the number of satisfactory observations to sodium, zinc, mercury and cadmium. Difficulty was also met with in getting readings upon lines lying near either end of the visible spectrum.

The following are the data taken:

Sodium. *Yellow lines* $(D_1 D_2)$.—The probable linear relation of $\lambda - \lambda'$ to H can be derived from Fig. 21, curve B, by drawing a line from the origin tangent to the curve. This is given in Fig. 24.

Zinc. *Blue line.* $\lambda = 4810.724$. (For arrangement of apparatus, see Fig. 10.)

TABLE VII.

H. C. G. S.	Δ mm.	$H \times \Delta$
4,000	10.2	408
5,000	8.30	415
6,000	6.80	408
7,000	5.80	406
8,000	5.05	404
9,000	4.40	396
9,500	4.15	394
10,000	3.95	395
10,500	3.85	405

Aver. = 403.

The average of column three is the product of $H \times \triangle$ of the equivalent hyperbola, and this, divided by any given value of H, gives a corrected value for $\triangle$, which may be used in determining the value of $\lambda - \lambda'$. In this way a double check is secured, first, in drawing a smooth curve through the original data, and thus getting the readings of column two; and second, in "averaging" these readings in the manner shown.

In Table VII. the average of column three is 403; the value of $\triangle$ corresponding to $H = 5{,}000$, is 8.06, and $\lambda - \lambda'$ is 0.144 A. U. This is given in Fig. 21.

Mercury.—Readings were obtained on the yellow, green and violet lines as follows:

TABLE VIII.

H	Yellow line. $\lambda = 5790.49$.		Green line. $\lambda = 5460.97$.		Violet line. $\lambda = 4358.56$.	
	$\triangle$	$H\triangle$	$\triangle$	$H\triangle$	$\triangle$	$H\triangle$
3,000	20	600	. .	. .	. .	. .
4,000	15.	600	12.0	480	. .	. .
5,000	11.9	595	9.5	475	. .	. .
6,000	9.85	591	8.0	480	. .	.
7,000	8.25	578	6.8	476	5.7	399
8,000	7.00	560	5.8	464	4.8	384
9,000	6.20	558	5.2	468	4.3	387
10,000	5.70	570	4.8	480	3.8	380
11,000	5.45	560	4.7	517	3.5	385
		Aver. = 579.		Aver. = 480.		Aver. = 387.

The magnetic shift for $H = 5{,}000$ is, yellow line 0.128 A. U., green line, 0.155 A. U., violet line 0.120 A. U.

Lines are then drawn through zero and this value, thus giving the magnetic shift for any value of $\triangle$. This is done on Fig. 22.

Cadmium.—Readings were taken on the red, green and blue lines as shown in Table IX., and platted on Fig. 23.

TABLE IX.

H	Red line. λ = 6438.9.		Green line. λ = 5086.3.		Blue line. λ = 4800.	
	Δ	*H* × Δ	Δ	*H* × Δ	Δ	*H* × Δ
3,000	.	. .	15.8	475	13.9	418
4,000	18.6	745	13.	520	10.7	432
5,000	14.15	708	10.8	540	8.4	420
6,000	11.4	685	9.	540	6.85	411
7,000	9.8	685	7.8	546	5.9	413
8,000	8.75	700	7.	560	5.3	424
9,000	8.	720	6.6	594	. .	. .

Aver. = 707. Aver. = 539. Aver. = 420.

The magnetic shift for $H = 5{,}000$ is red line, 0,131 A. U.; green line, 0.120 A. U.; blue line, 0.137 A. U.

The results may be summarized as follows :

TABLE X.

Substance.	Line.	Magnetic Shift for $H = 5000$	Magnetic Shift for $H = 10{,}000$
		A. U.	A. U.
Sodium.[1]	Yellow line D_1.	0.207	0.414
Mercury.	Yellow line.	0.128	0.256
"	Green "	0.155	0.310
"	Violet "	0.120	0.240
Cadmium.	Red "	0.131	0.262
"	Green "	0.120	0.240
"	Blue "	0.137	0.274
Zinc.	Blue "	0.144	0.288

This table is shown graphically on Fig. 24.

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[1] The echelon shows that the separation of the components of D_2 to be about two-thirds of that of D_1. The value here found belongs to D_1, the line having the greater magnetic shift.

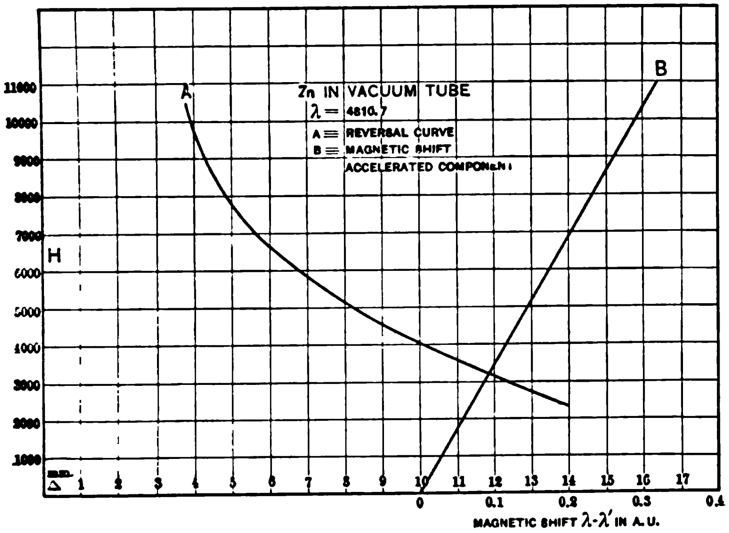
Zn IN VACUUM TUBE
λ = 4810.7
A ≡ REVERSAL CURVE
B ≡ MAGNETIC SHIFT
ACCELERATED COMPONENT
A
B
H
11000
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000
mm.
Δ
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17
0 0.1 0.2 0.3 0.4
MAGNETIC SHIFT λ-λ′ IN A.U.

Fig. 21.

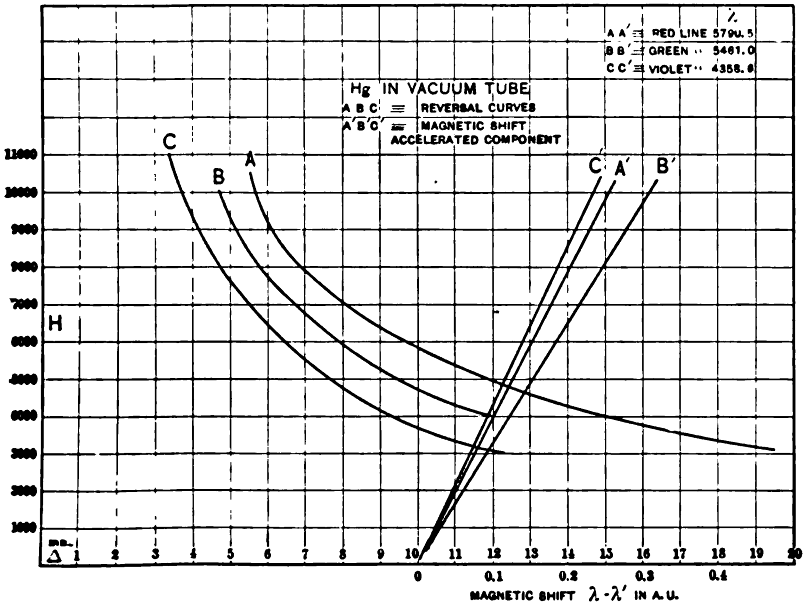
Hg IN VACUUM TUBE
A B C ≡ REVERSAL CURVES
A′B′C′ ≡ MAGNETIC SHIFT
ACCELERATED COMPONENT
λ
A A′ ≡ RED LINE 5790.5
B B′ ≡ GREEN " 5461.0
C C′ ≡ VIOLET " 4358.6
C
B
A
C′
A′
B′
H
11000
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000
mm.
Δ
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
0 0.1 0.2 0.3 0.4
MAGNETIC SHIFT λ-λ′ IN A.U.

Fig. 22.

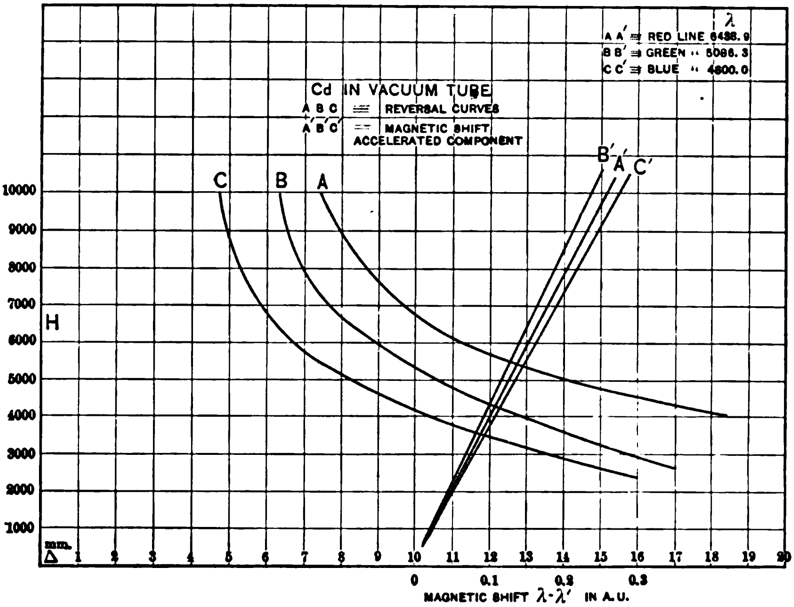
λ
A A′ = RED LINE 6438.9
B B′ = GREEN " 5086.3
C C′ = BLUE " 4800.0
Cd IN VACUUM TUBE
A B C == REVERSAL CURVES
A′ B′ C′ == MAGNETIC SHIFT
ACCELERATED COMPONENT
C
B
A
B′
A′
C′
H
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000
mm.
Δ
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20
0 0.1 0.2 0.3
MAGNETIC SHIFT λ-λ′ IN A.U.

Fig. 23.

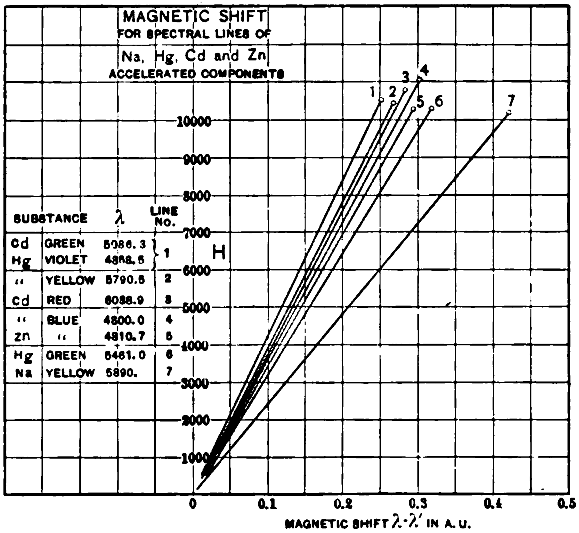
MAGNETIC SHIFT
FOR SPECTRAL LINES OF
Na, Hg, Cd and Zn
ACCELERATED COMPONENTS
SUBSTANCE
λ
LINE NO.
Cd GREEN 5086.3
Hg VIOLET 4358.5
1
" YELLOW 5790.5 2
Cd RED 6438.9 3
" BLUE 4800.0 4
Zn " 4810.7 5
Hg GREEN 5461.0 6
Na YELLOW 5890. 7
H
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000
1
2
3
4
5
6
7
0 0.1 0.2 0.3 0.4 0.5
MAGNETIC SHIFT λ-λ′ IN A.U.

Fig. 24.

RATIO OF IONIC CHARGE TO IONIC MASS.

Equation (10) gives the relation

$$e/m = \frac{\lambda - \lambda'}{\lambda^2} \frac{4\pi v}{H} \qquad (10)$$

for the case of circularly polarized light. This must be slightly modified for plane polarized light as the period of single vibration will be half that of the complete vibration of the circularly rotating ion. Equation (10) will then be written

$$e/m = \frac{\lambda - \lambda'}{\lambda^2} \frac{2\pi v}{H} . \qquad (19)$$

The numerical value in electromagnetic units may now be calculated from the values of $\lambda - \lambda'$ and H.

For the lines so far examined the values are as follows: The lines may be grouped according to the value of e/m; this gives the following interesting table.

TABLE XI.

Substance.	Line.	e/m	Type of line. (Michelson.)
Sodium.	Yellow.	22.45×10^5	Type II.
Mercury.	Vio'et.	23.81 “	
Cadmium.	Blue.	22.41 “	
Zinc.	Blue.	23.46 “	
Mercury.	Green.	18.59 “	Type III.
Cadmium.	Green.	17.48 “	
Mercury.	Yellow.	14.35 “	Type I.
Cadmium.	Red.	11.93 “	

It is thus seen that the groupings according to Michelson's chart (see Fig. 3) and according to the value of e/m are the same.

The data furnished by Preston[1] are too meager to furnish a just comparison, but it is to be said that his classification is not so well defined as is that furnished by the interferometer.

Polarization.—The method of observing the state of polarization has been outlined. In the lines examined the state of polarization was found to be normal with the exception of two cases, that seem deserving of notice.

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[1] Phil. Mag. (5), 45, p. 330. See part I.

I. *Violet Line of Mercury.*—In the readings of Table VIII. the settings were taken successively on the yellow, green and violet lines for the same value of H, by shifting the slit S in Fig. 10. The nicol was so set as to cause the fringes to expand, thus giving the retarded component. For high values of $\triangle$ the behavior of the fringes in the case of faint lines is not readily observed, and it was not until the value $\triangle = 5.14$ mm. was reached that it was noticed that the violet line contracted, while the yellow and green lines expanded. This behavior was carefully observed for the balance of the readings and persisted throughout. This peculiar state of polarization would indicate that the component of the violet line which *agrees* in rotation with the magnetic whirl is retarded instead of accelerated, as in the other lines. Hence we must have present a species of *diamagnetic* ion in contrast to the ordinary or "magnetic" ion. The above observations were taken with the greatest of care, and yet the conclusion seemed so unusual as to call for repeated verification. All attempts, however, to secure the same observations proved unavailing so far as reversed polarization was concerned.

II. In the case of the cadmium lines the following observations were made: The *red*, *green* and *blue* lines were examined, the nicol being set so as to cause the fringes to contract, thus giving the accelerated ray. The observations were begun with $H = 2{,}900$ C. G. S. units. At the second reading with $H = 6{,}600$ the fringes *all* expanded, and so also for $H = 7{,}900$. At the last reading with $H = 8{,}800$ *all* the fringes again contracted. The apparent conclusion to be drawn is that a reversal of the state of polarization takes place during the progress of the experiment. It is more likely, however, that in this case, since (as already stated) the glass of the vacuum tube acts as a retardation plate, some change of stress due either to variation in temperature or to the magnetic field, is responsible for the observed effect. This fact, in connection with the difference in wave-length between the violet and the green and yellow lines of mercury, may, perhaps, also explain the unusual behavior observed in this line. Both these cases illustrate the necessity of carefully watching the *instrumental factors* present.

Summary.—The study of the magnetic shift and the values of e/m lead to the following conclusions :

I. A classification of lines according to the amount of magnetic shift is of little value.

II. A classification of lines according to the value of e/m is significant.

III. Such a classification groups the lines according to the type of line produced, as given by the analysis of visibility curves.

IV. The smaller the value of e/m the less the broadening of the component lines and the simpler their structure; *vice versa* the larger the value of e/m the more the broadening, and the more complex their structure.

RECAPITULATION.

In the preceding pages the history of the subject has been traced, from the first experiments of Faraday to the present time. The bearing of the Maxwell-Lorentz theory upon the progress of Zeeman's experiments is noted, and the reacting influence of experiment upon theory is shown, so that in the papers of Larmor, Lorentz and others the mathematical exposition of the subject has kept pace with the experimental observations. In the experimental development of the problem two well-marked methods have been presented and compared. It has been found that the spectro-photographic method is (1) limited in range by reason of the small resolving power of the ruled grating when so minute changes are to be measured; and (2) is limited in accuracy by reason of the wide margin of error in the settings of the micrometer, especially when nebulous lines are to be measured.

On the other hand, the interferometer method has been seen to possess a resolving power greatly in excess of the photographic method, and hence is applicable to low values of H as well as to high values.

The results accomplished so far consist in

I. By the spectro-photographic method.

(1) A classification of lines according to the type of line produced by the magnetic field.

(2) The measurement of the distance between outside components of magnetic triplets and a determination of the value of the ratio e/m.

The number of lines so far examined is not large, nor do the values of the magnetic shift obtained by different observers seem to agree very well; but enough has been done to suggest a classification of lines parallel to that obtained by Kayser and Runge.

II. With the interferometer Professor Michelson has presented,

(1) Three well-marked types of lines, with a possible fourth type.

(2) He has concluded that the magnetic shift is to be regarded as approximately independent of substance or color.

The chief value to be attached to his work is contained in (1), and it would seem to us better to leave (2) unformulated rather than to state it as an approximate law.

The experiments described in this paper have sought to determine,

(1) The relation, at different temperatures, of the magnetic shift to strength of field.

(2) To present a method of measuring the magnetic shift that shall be as free as possible from objections involved in existing methods.

The method adopted is similar to existing interferometer methods, but is believed to be especially adapted to the present problem.

(3) To measure the magnetic shift and determine the ratio e/m.

The values obtained for magnetic shift show that any classification based upon this alone is of relatively small value; but that a classification based upon the value of e/m is significant.

This classification is seen to give groups of lines identical with the groups presented by Michelson's three types of lines.

The complexity of structure is also seen to depend upon the value of this ratio e/m.

The preceding experiments were carried on in connection with the Fellowship in Physics from 1897 to 1899 at the University of Wisconsin, under the direction of Professor Benjamin W. Snow; whom, in conclusion, I desire to thank for his helpful and continued encouragement throughout the progress of this investigation.

PHYSICAL LABORATORY, UNIVERSITY OF WISCONSIN, April, 1899.

APPENDIX I.

Extract from Life of Faraday by Dr. Bence Jones, Vol. II., p. 44.

"1862 was the last year of experimental research; Steinheil's apparatus for producing the spectrum of different substances gave a new method, by which the action of magnetic poles upon light could be tried. In January he made himself familiar with the apparatus, and then he tried the action of the great magnet on the spectrum of NaCl, BaCl, StCl and LiCl.

"On March 12 he writes:

"'Apparatus as on last day (January) but only ten pairs of voltaic battery for the electromagnet.

"'The colorless gas flame ascended between the poles of the magnet, and the salts of sodium, lithium, etc., were used to give color. A Nicol's polarizer was placed just before the intense magnetic field, and an analyzer at the other extreme of the apparatus. Then the electromagnet was made, and unmade, but not the slightest trace of effect on or change in the lines of the spectrum was observed in any position of polarizer or analyzer.

"'The other pierced poles were adjusted at the magnet, the colored flame established between them, and only that ray taken up by the optic apparatus which came to it along the axis of the poles, *i. e.*, in the magnetic axis or line of magnetic force.

"'Then the electromagnet was excited and rendered neutral, but not the slightest effect on the polarized or unpolarized ray was observed.'

"This was the last experimental research that Faraday made."

II.

Extract from M. Ch. Fievez (Astronome a l'Observatoire Royale de Bruxelles), *Bulletins de l'Academie Royale de Belgique*, 3d serie, tome ix., p. 381 (1885).

"L'installation spectroscopique de l'Observatoire, disposant d'un appareil dispersif de tres grande puissance et d'un electro-aimant Faraday, construction Ruhmkorff, pouvant etre active par un courant de 50 ampres d'intensite, a permis d'aborder ce probleme.

"La flamme oxyhydrique d'un petit chalumeau etait dirigee horizontalement sur un charbon *sode* place entre les armatures coniques de l'electro-aimant, distantes l'une de l'autre de 10 millimetres. Une image de la flamme etait projetee sur la fente du spectroscope par un objectif double. La quantite d'oxygene introduite dans cette flamme permettait de regler la temperature de facon a donner aux raies spectrales D_1 et D_2 l'apparence voulue.

"Dans ces conditions, les raies sodiques D_1 et D_2 etant d'abord peu larges et non renversees avant le passage du courant d'aimantation, deviennent immediatement *plus brillantes, plus longues et plus larges* aussitot que l'electro-aimant est mis on activite.

"Si les raies brillantes D_1 et D_2 sont deja elargies, l'electro-aimant etant inactif, elles deviennent plus larges encore et *se renversent* (c'est-a-dire qu' une raie noire parait au milieu de la raie brillante elargie) pendant le passage du courant d'aimantation.

"Si les raies sont deja elargies et renversees, l'elargissement de la raie brillante et de la raie noire devient beaucoup plus considerable.

"Ces phenomenes, *qui disparaissent instantanement lors de l'interruption du courant,* peuvent etre observes, mais avec moins d'intensite, sur la raie rouge du potassium, du lithium, sur la raie verte du thallium, etc., lorsqu' une minime quantite de ces metaux ou d'un de leurs sels est placee sur le support de charbon.

"Enfin, les armatures coniques de l'electro-aimant etant remplacees par les armatures meplates, de maniere que toute la longueur de la flamme sodique seit comprise entre ces armatures, les raies D_1 et D_2, prealablement renversees et elargies, presentent un *double renversement* (c'est-a-dire l'apparition d'une raie brillante au milieu de la raie noire elargie), lorsque l'electro-aimant est en activite."

III.

Extract from article in Astro. Phys. Jr., February, 1899, by Perot and Fabry.

"* * * when the light is complex it is easy to obtain a precise measure of the ratio of the wave-length of the radiations which constitute it; let there be two radiations of nearly equal wave-length λ and $\lambda - \epsilon$. The distances between the silvered surfaces is in-

creased until the discordance between the two systems of rings is complete. Then if e is the distance between the surfaces (which is given with sufficient accuracy by the micrometer) we have

$$\frac{\varepsilon}{\lambda} = \frac{\lambda}{4\varepsilon}$$

"* * * This method is also readily adapted to the study of the change of wave-length of a given line, on condition that the radiation be sufficiently monochromatic; in such a case a comparison can be made of two sources emitting, for instance, in the one case the altered radiation and in the other the normal radiation, attention being directed to the change in the appearance of the rings produced by the two sources successively."

* * * * * * * * * *

NOTE.—With the interferometer the ray travels the distance between the plates twice and hence $\Delta = 2e$. Therefore the equation for the interferometer becomes

$$\frac{\varepsilon}{\lambda} = \frac{\lambda}{2\Delta} \text{ or } \varepsilon = \frac{\lambda^2}{2\Delta}.$$

THE EFFECT OF MAGNETIZATION UPON THE ELASTICITY OF RODS.

BY J. S. STEVENS AND H. G. DORSEY.

IN the PHYSICAL REVIEW, Vol. II., No. 4 and Vol. III., No. 6, is described a series of important experiments which show the relation between temperature and elasticity in a wire. In one of the papers the statement was made that the results seem to indicate that the magnetizing effect of the current through the wire increases the modulus of elasticity. The increase in temperature in the wire was produced by sending a current directly through it, and also by sending a current through the helix which surrounded it. The author stated that "the magnetization produced by the first method had no appreciable effect on the result, and that if there is any difference in the effects produced upon the elasticity of a wire by magnetizing it, that difference is too small to be detected with any certainty by this experiment."

This paper contains the results of some experiments whose object was to test the effect of magnetization upon the modulus of elasticity by the application of interference methods of measurement.

The accompanying figure will make clear the method of operation. A series of carefully prepared rods[1] whose dimensions are given below, were in turn supported in the ordinary manner for determining the elastic modulus at points 3.2 cm. from their ends on adjustable knife-edges. In the center of the rod suspended from a hook was the load causing the deflection, and on the upper surface one of the mirrors of an interferometer. Surrounding the rod was first an air space, then a chamber for the passage of a stream of water, and on the outside two series of coils having a resistance of

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[1] The same rods were used by Mr. C. P. Weston in his experiment described in the PHYSICAL REVIEW for May, 1899.

54.4 ohms each. When the rod was bent by the deflecting weight the interference fringes were found in the usual manner. A sodium lamp was used as the source of light radiations. It is obvious that any change in the modulus of elasticity due to magnetizing the bar would produce a corresponding motion in the fringes seen in the mirror.

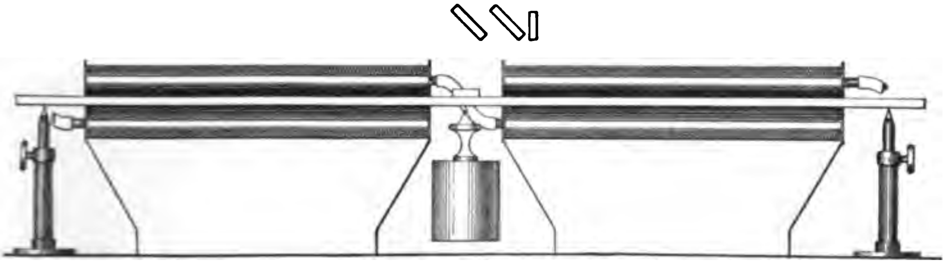

Fig. 1.

The great source of error to be guarded against in this work was the effect on the modulus due to heat. That this was eliminated will, we think, appear from the following considerations:

(*a*) A stream of water was caused to run through the chamber between the coils and the rod while the observations were made.

(*b*) The motion of the fringes appeared to be instantaneous when the current was turned on. When the circuit was kept closed for some time a creeping motion of the fringes (probably due to temperature changes) was observed.

(*c*) A copper bar of the same dimensions as those previously used was substituted, and no such instantaneous motion of the fringes was observed.

(*d*) The fringes came back to their initial position immediately when the current was turned off, when the steel rod was used.

(*e*) A thermometer reading by estimation to hundredths of a degree showed no appreciable change of temperature in the air surrounding the rod while the displacement of the fringes was being measured.

Among the difficulties encountered were:

(*a*) The arrangement of the mirrors so as to get good fringes. The conditions for producing interference were extremely unfavorable as the adjustment of the mirror on the bar had to be made entirely by hand. It was supported on wax of such a consistency that it could be easily moved, but when once adjusted it kept its place. The difficulty here referred to, however, was one which needed only practice and patience to overcome, and whenever the observations were recorded the fringes were sufficiently distinct.

(*b*) Although the apparatus was mounted in a basement laboratory, on a stone slab supported by a brick foundation which did not come in contact with the floor or any other part of the building, a clock striking four stories above it caused a rhythmical motion of the fringes. It may be judged from this illustration that the work had to be done at such times as we could find the building free from ordinary disturbances.

(*c*) Another difficulty was encountered at first arising from the use of homogeneous light, and the consequent similarity of the fringes. With the mirror adjustments used here it would have been next to impossible to have obtained the colored fringes, which may readily be identified; but it was soon found that the displacements were never of an order of magnitude greater than one wave-length, and therefore the necessity of identification was avoided.

The following data were employed:

Length of rods,	66.3	cm.
Distance between knife-edges,	60.0	"
Breadth of rods,	1.0	"
Thickness of rods,	0.54	"
Length of magnetizing helix,	53.0	"
Number of layers of wire,	9.6	
Number of turns of wire,	7026.	
Minimum current used,	0.12	amp.
Maximum current used,	0.82	"
Number of turns per cm.,	132.	

In the tables which follow are shown the current, the magnetizing force, the intensity of magnetization and the accompanying displacement of the fringes. In every case the direction of displacement indicated an *upward* movement of the mirror, and hence an *increase* in the modulus of electricity.

TABLE I.

STEEL BAR. LOAD 1K.

Current in amperes.	Magnetizing force.	Intensity.	Displacement in wave-lengths.
0.46	77.5	A nearly uniform intensity of about 700.	0.06
0.54	89.6		0.15
0.57	93.8		0.20
0.60	99.6		0.25
0.64	106.2		0.38
0.66	108.7		0.45
0.68	112.9		0.50
0.71	117.9		0.50
0.75	124.5		0.63

TABLE II.

STEEL BAR. LOAD ½ K.

Current in amperes.	Magnetizing force.	Intensity.	Displacement in wave-lengths.
0.44	73.0	A nearly uniform intensity of about 700.	0.06
0.45	74.6		0.06
0.46	77.5		0.06
0.62	102.9		0.13
0.64	106.2		0.19
0.65	107.9		0.25
0.66	109.6		0.25
0.68	112.9		0.31
0.71	117.9		0.50
0.75	124.5		0.50

TABLE III.

WROUGHT IRON BAR. LOAD 1 K.

Current in amperes.	Magnetizing force.	Intensity.	Displacement in wave-lengths.
0.12	19.9	389.7	0.08
0.15	24.9	516.5	0.22
0.27	44.9	735.2	0.28
0.33	54.1	838.2	0.35
0.38	61.3	....	0.38
0.54	89.7	....	0.48

TABLE IV.

WROUGHT IRON BAR. LOAD ½ K.

Current in amperes.	Magnetizing force.	Intensity.	Displacement in wave-lengths.
0.16	26.6	615.1	0.20
0.33	54.8	730.0	0.50
0.43	71.6	838.2	0.63
0.60	99.6	868.8	0.75
0.76	125.9	1075.0	0.88

DISCUSSION OF RESULTS.

The difficulties encountered in making these observations, and the fact that it was necessary to make them quite rapidly conspired to render the results recorded in the last columns of the tables rather unreliable as quantitative measurements. So far as practicable, however, they represent the mean of several observations taken by each of us. If they were plotted either with the magnetizing force or the intensity of magnetization they would not yield a straight line or any other regular curve. In every case, however, an increase of current was accompanied by an increased displacement. In the case of the steel bar it was necessary to keep the current within a limited range, and as shown in the tables the intensity remained nearly constant. With the wrought iron we were able to use a greater range of current and secured a greater deviation in the displacement column.

The following may be set down as the results of the experiment:

(*a*) The modulus of elasticity of wrought iron and steel increases with magnetization.

(*b*) In the case of steel little difference was observed for loads of 1 K and ½ K. In the case of wrought iron the smaller load gave the greater displacement when equal magnetizing forces were used.

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ON FREEZING AND BOILING WATER SIMULTANEOUSLY.

By R. W. Quick.

MANY lecturers in physics perform annually the well-known experiment of placing a small quantity of water in a shallow dish under the receiver of an air pump and by exhaustion reduce the pressure below the vapor pressure of ice at 0°C. or 0.46 cm. when, if everything works properly, the water will be seen to boil and freeze at the same time, the lowering of the temperature being caused by rapid evaporation.

But this experiment requires a pump with automatic valves to attain sufficient exhaustion, the very best of thermal insulation from the bed-plate of pump, and some drying agent to absorb the vapor; and even under the best of conditions the quantity of liquid used must be so extremely small as to render the experiment not easily seen by a class.

In a recent lecture to a class in heat I accomplished the experiment in a manner which, I think, is novel and which is infinitely more satisfactory. The great advantage in this method is the large quantity of water which can be used, and the violence with which the ebullition occurs simultaneous with its freezing.

A flask made of good, strong, thin glass of about 500 c.c. or a liter capacity is nearly filled with water, and its contents boiled for at least an hour so as to insure the removal of all air from the flask. While still boiling violently the flask is quickly corked with a rubber stopper and the flame immediately removed. After inverting the flask cold water is poured on it, causing the water to boil. This will continue till the temperature of the water in the flask reaches a point nearly equal to that of the water applied. (This part of the experiment is a well-known one.) If ice water be applied to the flask the water in the flask will continue to boil till its temperature reaches about 3° C. Of course, there is no "boiling away," as the vapor, after it is formed, is again condensed when it strikes the cold walls of the flask. In this experiment it is to be observed that the fall in temperature of the water in the flask is not due to evaporation, but is solely accomplished by the cold liquid applied to the outside of the flask.

The process is continued from this point by the application of a cold liquid. The liquid I used was ether in which was placed a large quantity of solid carbon dioxide which can easily be obtained from the commercial carbonic acid gas contained in cylinders under 55 atmospheres of pressure, and used for charging soda fountains. When a sufficient quantity of the solid is placed in the ether it assumes a constant temperature of — 75° C. It is necessary to use only about 100 c.c. of ether contained in a 150-c.c. glass beaker. Pouring this liquid upon the inverted flask, which must be supported in position by wood supports, and allowing the liquid to run down the neck of the flask, the water begins to boil immediately and continues to do so as the temperature of the water falls to the freezing point. This occurs in a few seconds after the cold liquid is applied. At this point steam will be seen to freeze on the walls of the flask, making it appear that the flask is cracking. Soon after bubbles of steam will be seen to have an envelope of ice formed about them just as they break through the surface of water and in a short time a layer of ice forms on the water through which bubbles of steam attempt to pass on rising to the surface. Ice also forms at the neck of the flask.

At this point I ceased pouring on the ether for fear the flask would burst due to the freezing of water in its neck.

Of course it is of the utmost importance to have all the air expelled from the flask before corking, else the pressure might be above 0.46 cm., in which event no amount of cooling would cause the water to boil and freeze simultaneously.

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NEW BOOKS.

Scientific Papers. By P. G. Tait, Sec. R. S. E. Vol. I. Cambridge University Press. New York, The Macmillan Company.

The publication, under the auspices of a great university, of the collected works of a scientific man, is in itself a sufficient indication of the distinguished character of his investigations and of the high regard in which they are held by competent judges. The field that Tait has occupied is in one respect unique. His writings cover indeed, a wide range in mathematical and physical subjects, and his name will always be associated with those of the most notable workers in the modern development of physical science and physical ideas, but he also stands out prominently as the great successor to Hamilton in the development of quaternion analysis. A large portion of his investigations has been conducted by the aid of that analysis and have resulted not only to the advantage of the subject itself, but have also imparted to physical subjects new ideas and developments that are constantly employed even by those who have no knowledge of quaternions. It is hardly possible for the ordinary analyst to estimate the influence which the development of quaternions has had upon the mathematical and physical conceptions of the time. It has, indeed, given them an entirely new aspect and character, and has very greatly contributed by its natural methods of attacking problems, to the removal of the scientific disease of phasia or want of direct and visible apprehension. In all the developments of Tait, the physical and visual side remain predominant, and it is because of this natural desire to see things as they are, that the quaternion methods have appealed to him most strongly. Of the articles in this volume that are especially noteworthy as developments both of quaternion and physical truths, we may note those on Fresnel's wave-surface, on electrodynamics and magnetism, and the potential of a closed circuit, on small displacements of the particles of a medium, on the rotation of a rigid body about a fixed point, on Green's and other allied theorems, on linear differential equations in quaternions, on orthogonal isothermal surfaces, etc. The important results in all these papers are put in such shape as to be comprehensible to the ordinary analyst and physicist, for it is the peculiar property of quaternion forms that they are intelligible to all when stated in words, involving as they do, only direct processes of geometry and physics. There are also valuable papers on many mathematical and physical subjects that employ the quaternion analysis very little or not at all, such as first approximation to a thermo-electric diagram, on electrolytic polarization, on the law of frequency of error, on thermal and electric conductivity, on mirage, the pressure errors of Challenger thermometers, etc. The volume also contains the complete series of papers on Knots. These constitute the most important work in this subject, first considered by Listing. In order to bring these papers together, the practice of publishing the different papers in the order of their presentation to the public, has not been followed, so that under the title of Knots we have several long papers, some of which would, in their order of time, appear in succeeding volumes. The volume is interspersed with valuable and brief notes, and also with lectures and addresses on various subjects, which are interesting and have often been epoch-making in respect to the ideas involved.

In editing his own writings at the request of the Syndics of the University Press, Tait has neglected to note those papers that received prizes for special merit at the time of their publication. For example, the Keith Prize for the best communication on a scientific subject, communicated in the first instance to the Royal Society of Edinburgh (preference given to a paper containing a discovery), was awarded for the period 1867–69 to the paper, "On the Rotation of a Rigid Body About a Fixed Point," and for the period 1871–73 to the paper, "First Approximation to a Thermo-electric Diagram." These two papers are typical illustrations of the two lines of work pursued by Tait, and a brief synopsis of them will show the general character and trend of his different investigations, which are always purely physical, whether in quaternion or analytical form.

The purpose of the first paper is best explained by a brief quotation from the author's introduction: "Although it is very improbable that there remains to be discovered any new, and at the same time simple, fact connected with a question which has been elaborately treated by many of the greatest mathematicians of this and the preceding century, the employment of a new mathematical method may enable us to present some of their results in a more intelligible form, and with far less expenditure of analytical power than has hitherto been deemed necessary; and it may give us such an insight into the question that we shall be able easily to discover the mutual relations among the various processes which have been employed; so far, at least, as these differ in principle, and not merely in the peculiar co-ordinates assumed for the purpose of simplifying the equations. Such a method is that of *Quaternions*, which seems to be expressly fitted for the symmetrical evolution of truths which are usually obtained by the ordinary Cartesian methods only after great labour of calculation, and by modes of attack so indirect, and at first sight so purposeless, as to bewilder all but a very small class of readers."

The fundamental basis of the investigation is the almost instantly demonstrated property that "*if q be any quaternion, the operator of $q\,(\;)\,q^{-1}$ turns the vector, quaternion or system, to which it is applied, about the axis of q through double the angle of q.*"

If α be the vector number of a line drawn from the fixed point to any point of the body in its initial position, and ω be the vector number of the same line at time t, it follows at once that $\omega = q\alpha q^{-1}$, where q is some function of the time.

The differentiation of ω as to the time gives

$$\dot{\omega} = V.\,\varepsilon q\alpha q^{-1}, \text{ where } \varepsilon = 2V\dot{q}q^{-1}.$$

This shows that the line whose number is ε is the axis of instantaneous rotation and that $T\varepsilon.dt$ is the amount of instantaneous rotation. The expression of these results in terms of any coördinates of the line ω is now simply a matter of substitution of the resulting value of q, multiplying out, and equating corresponding coefficients of i, j, k. The formulas of Rodrigues and Cayley are thus found by simple multiplication, and similar forms in other coördinate systems, some of which are simpler than those of Cayley.

In considering the motion of the mass, Lagrange's general equations of motion take at once the form $\Sigma \cdot mV\omega\ddot{\omega} = \psi$, where m is the element of mass to which ω refers, and ψ is the vector couple acting at time t. This gives the integral

$$\Sigma \cdot mV\omega\dot{\omega} = \int\psi dt, \text{ or } \Sigma m\omega V\varepsilon\omega = \gamma.$$

When no forces act, γ is a constant vector, and from this fact follows at once Poinsot's beautiful construction of the motion by the rolling of a central ellipsoid fixed in the body on an invariable plane. Some very simple eliminations give the above equations in the form

$$2\dot{q} = q\varphi^{-1}\,(q^{-1}\gamma q)$$

which is a differential equation for the unknown quaternion q that fixes the position of the body at any time t. φ is a linear vector function depending only upon the initial position of the body and the distribution of mass in that position, and γ is the supposedly known couple acting at time t.

Tait says, "It is hardly conceivable that any simpler or more easily interpretable equation for q can be presented until symbols are devised far more comprehensive in their meaning than any we yet have."

The strain function φ is shown to pertain to the momental ellipsoid of the body, so that if i, j, k are units along the principal axes of the body in its initial position, and A, B, C, the movements of inertia about those axes, we have

$$\varphi\rho = -\,(AiSi\rho + BjSj\rho + CkSk\rho).$$

The article continues with the discussion of the integration of the above equation, the determination of the equations that have resulted from the labors of Euler and others, and various new equations, the solution for a homogeneous solid of revolution about a fixed point in its axis not the center of gravity, the equations of precession and nutation, etc.

"First approximation to a thermo-electric diagram" is the culumination of a series of theoretical and experimental papers whose results have a standard value in the subject. The principal results are given in Maxwell's Electricity and Magnetism, Vol I., Art. 254. In an early paper Tait had deduced from considerations regarding dissipation of energy that what Lord Kelvin calls the specific heat of electricity in a conducting substance should be, like thermal resistance, directly proportional to the absolute temperature, being for some substances negative. With a view of testing this theoretical result a number of experiments were conducted for the determination of the electro-motive force in various thermoelectric circuits through wide ranges of temperature. The resulting curves were found in all cases to be excellent parabolas. This accorded with the theoretical conclusion above, which makes the second temperature rate of change of electro-motive force a constant. The paper gives a synopsis of investigations and experiments that led up to this result, and devises a thermo-electric diagram, whose abscissas represent the absolute temperature of one junction of two metals, the other being maintained at constant temperature, and whose ordinates represent the temperature rate of change of the electro-motive force at the given temperature. The corresponding curves should therefore be straight lines, but in order to compare all substances with each other, they are taken with reference to a certain substance whose specific heat of electricity is zero, and which is therefore represented by the axis of abscissas. The experiments of Le Roux seem to show that lead is very approximately such a substance. The lines of the diagram then show the rate of charge of electro-motive force of the various substances in circuit with lead, and should be very approximately straight lines of positive or negative slope corresponding to their specific heat of electricity coefficient. The difference between two ordinates at any temperature is then the rate of change of electromotive force in a circuit between them, at that temperature. The rectangle of this difference and the abscissa of absolute temperature is the Peltier effect at the junction. The intersection of the two lines is therefore at the temperature of the so-called neutral point. The electro-motive force between two metals when its junctions are at different temperatures is proportional to the difference of the areas of the triangles formed by the diagrammatic lines of the substances and the ordinates at the two temperatures.

One peculiarity of the diagram is that certain metals, for example nickel, have a broken diagram, the separated portions being very approximately straight lines, which seems to show, as Tait remarks, that some bodies take successively different states at certain critical temperatures, retaining their thermo-electric properties nearly unchanged from one of those critical points to another. The experiments indicate such a critical temperature for iron at a low red heat, and Tait calls attention to the fact that other changes may be expected to take place in iron at this temperature besides the above thermo-electric one, and the sudden change of length of iron wire at this temperature observed by Gore and the sudden glow observed by Barrett that occurs simultaneously.

It would give me pleasure to outline many papers in this volume and to point out the gain in brevity, clearness, and simplicity of treatment that results from the use of the quaternion analysis, but I have already reproduced enough to indicate the distinctive features of the volume, and can only say in conclusion that I hope it may happen to others as it has often happened to myself, that the quaternion investigations of Professor Tait have been a revelation as to the true meaning of the physical part of the subject.

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Physical Problems and their Solutions. By A. BOURGOUGNON. Pp. iv+224. New York, D. Van Nostrand Company, 1897.

This is a collection of 215 simple problems in physics, to each one of which the numerical solution is given in full The subjects of electricity and magnetism are excluded altogether, so far as problems are concerned; there being merely a brief statement of a few fundamental laws, such as Coulomb's law and Ohm's law at the end of the volume. British measures and the metric system are used indiscriminately throughout, a bad practice for which the author may, however, find his warrant in the example set by the Regents of the University of the State of New York. Some of the problems contained in this volume are indeed taken from their papers, and in the appendix a special list of "*Regents' questions*" is given.

E. L. N.

Ostwald's Klassiker der Exakten Wissenschaften. Nos. 61, 63, 72, 75, 76, 79, 80, 81, 82, 83. Leipzig, Wilhelm Engelmann, 1895 and 1896.

We have before us the above numbers of the series of Ostwald's reprints and translations of the classics of the exact sciences. In these days when every scientific man reads French, German and English with equal ease and is able in an emergency to translate for himself the substance of papers published in Dutch, Danish, Swedish and Italian, one cannot but wonder that scientific men should find the time to translate into a single language the numerous monographs included in Ostwald's series. There is, however, no one of the treatises included in the list before us which is not well worth being reprinted and, for that matter, of being translated into German, and the thanks of the scientific world at large are certainly, due to any one who is willing to undertake the task of collecting and reissuing so large a number of valuable papers and thereby rendering them more accessible than they have been in the past.

E. L. N.

Volume IX. *September, 1899.* *Number 3.*

THE

PHYSICAL REVIEW.

A NEW RESPIRATION CALORIMETER AND EXPERIMENTS ON THE CONSERVATION OF ENERGY IN THE HUMAN BODY. I.

By W. O. Atwater and E. B. Rosa.

Contents.

I. Introduction.
  - Development of the investigations.
  - Metabolism of matter and energy.

II. Apparatus and Methods.
  - Outline of apparatus and plan of experiments.
  - Arrangements of the apparatus.
  - Arrangements for preventing gain or loss of heat through the walls of the calorimeter.
    1. Construction of the calorimeter.
    2. The thermo-electric elements.
    3. Heating and cooling appliances.
    4. The observer's table.
  - Apparatus and method for measuring the heat.
    1. The water system.
    2. The water meter.
    3. Determining the increase of temperature of the water.
    4. Determining the temperature of the calorimeter.
  - The ventilating air current.
    1. Measuring and regulating the temperature of the ingoing air.
    2. The refrigerating apparatus.
    3. The Blakeslee meter pump.
    4. Analysis of the air samples.
    5. Residual carbon dioxide and water vapor in the respiration chamber.

III. Tests of the Apparatus as a Calorimeter.
  - Electrical test experiments.
  - Alcohol test experiments.
    1. Composition of alcohol used. Specific gravity.
    2. Heat of combustion of alcohol. Determinations with the bomb calorimeter.

3. The calorie here used as the unit of measure.
4. The latent heat of water vapor.
5. Description of the test experiments.
6. Summary of the test experiments.

IV. EXPERIMENTS WITH MAN.
Plan of the experiments.
Explanation of the tables.
Results of the experiment with a human subject.
Mechanical efficiency of a man.
General summary.

ILLUSTRATIONS.

Fig. 1. General view of the calorimeter.
" 2. Horizontal section of the respiration calorimeter.
" 3. Thermo-electric junction.
" 4. Diagram of electric apparatus at the observer's table.
" 5. Interior of respiration chamber.
" 6. Section through walls of the calorimeter, showing thermometers.
" 7. Water meter.
" 8. Diagram illustrating use of the copper thermometer.
" 9. Air temperature regulator.
" 10. "Vestibule."
" 11. Refrigerating apparatus.
" 12. Blakeslee meter pump.
" 13. Arrangement of electrical apparatus for generating heat.
" 14. Bomb calorimeter.

I. INTRODUCTION.

THE purpose of this article is to describe a new respiration calorimeter and certain experiments made with it upon the conservation of energy in the human body. The apparatus has been devised and the methods of experimenting have been elaborated for use in inquiries upon the transformation of matter and energy in the living organism. This especial study is desirable for two purposes. One is the demonstration, so far as this may be possible, that the law of the conservation of energy holds in the living organism. The other is the practical application of this law in gaining more definite knowledge of the ways in which the body is nourished and of the values and uses of food. Preference has been given at the outset to the more theoretical problem of the conservation of energy, not only because of its profound scientific interest, but also because of its fundamental importance in the study of the laws of nutrition.

ASTOR, LENOX AND FOUNDATIONS.

DEVELOPMENT OF THE INVESTIGATIONS.

In the year 1892 the first steps were taken by the writers, at Wesleyan University, toward the development of a respiration calorimeter. The investigation was conducted under the patronage of the University, and in connection with the Storrs Experiment Station, of which Mr. Atwater is director. The progress of the work led to constantly increasing hopes of success, but at the same time showed more and more clearly the need of considerable amounts of labor and money in order to insure results at all commensurate with the importance of the inquiry. In 1894 provision was made by Act of Congress for an inquiry into the food and nutrition of the people of the United States. The responsibility for the inquiry was vested in the Secretary of Agriculture by whom it was assigned to the Director of the Office of Experiment Stations, and the immediate charge was placed in the hands of the Director of the Storrs Experiment Station as Special Agent of the Department of Agriculture. It was considered that a research, not only germane, but fundamental to such an inquiry might be appropriately aided from this fund, though the amount which could be utilized for this purpose was small. In 1895 the Legislature of Connecticut provided a special annual appropriation to be expended by the Storrs Experiment Station for food inquiries. The resources of the Station for this purpose were thus increased, and with the supplement from the general Government and with the aid from Wesleyan University and other sources, it has been possible greatly to enlarge the scope of this special investigation and to prosecute the work in a manner which would otherwise have been entirely out of the question.

The inquiry naturally divides itself into two parts. These have to do, as has already been stated, with the study of the law of the conservation of energy in the living organism on the one hand, and the study of the application of this law in the problems of animal nutrition on the other. That part of the inquiry which had to do with the establishment of the law of conservation of energy was undertaken jointly by the present authors. Mr. Rosa having recently retired from the investigation, the present article is intended to give an account of the development of the apparatus and methods and some of the experiments with a human subject up to the time of his retirement. We wish to acknowledge our indebtedness throughout the entire period of the work to our associates, all of whom have entered into the work with enthusiasm. Mr. A. W. Smith, who has been engaged since 1894 in the physical side of the investigation, has rendered invaluable assistance in carrying out the experiments here described.

To Mr. O. S. Blakeslee, college mechanician of Wesleyan University, we desire to express our obligation for the meter pump, which was devised and made by him, and is briefly described in the following pages, and also for much valuable assistance in the construction of the apparatus.

A more detailed account of the apparatus, and especially of the experiments described in this article may be found in Bulletin 63 of the Office of Experiment Stations of the United States Department of Agriculture, from which the most of the illustrations of this article are taken.

METABOLISM OF MATTER AND ENERGY.

In so far as its material phenomena are concerned, life consists of transformations of matter and energy. To these transformations the term metabolism is commonly and appropriately applied. The processes of metabolism are thus of two definite and closely allied kinds, the metabolism of matter, in which the changes are chemical, and that of energy, in which the changes are physical. It is commonly assumed that these two classes of change in living organisms, animal and vegetable, conform to the two fundamental laws of the conservation of matter and the conservation of energy.

The main difficulty in demonstrating that the law of the conservation of energy holds in the living body is in measuring the transformations of energy which take place therein. To do this we must know how much energy the body receives, how much it gives out and how much it gains or loses of its accumulated store during a given time. In an experiment with a man, for instance, if we wish to demonstrate that the law of the conservation of energy obtains we must show that the income and outgo are equal. To show this equilibrium of energy we must determine the following quantities:

(1) the amount of potential energy the body receives as income in the food; (2) the energy it gives off (*a*) as potential energy in the unburned material which it excretes (in urine, feces, etc.), (*b*) as kinetic energy in the heat radiated from the body, and (*c*) as external muscular work done; and, finally, (3) the increase or decrease in the store of energy in the body during the experiment. If the body is warmer, or if it has a larger store of proteids or fat or glycogen or other substance containing potential energy at the end than at the beginning it has stored energy during the period. If, on the other hand, it is colder, or its store of substance containing potential energy is less at the end of the experiment than at the beginning, it has lost energy. This gain or loss of energy by the body itself, the increase or diminution of its store during the experimental period, must be taken into account if the experiment is to be accurate and reliable. Its exact quantitative determination is one of the most serious difficulties in such experiments.

In view of what was said above it might seem bold to attempt experiments with animals, or even with a man, in the expectation that any convincing proof of the application of this law could be obtained, and it might seem still more hazardous to assume that apparatus and methods of experimenting could be devised which should make it possible not only to demonstrate the law but to utilize it in general experiments upon the laws of nutrition. The task would not have been assumed in the present instance had not an attempt at least seemed warranted by a somewhat unusual combination of circumstances through which appliances and funds were available from several distinct sources, as already indicated. Nor is it at all surprising that, even with the unusual facilities thus brought together, a period of more than five years should have elapsed between the beginning of the actual work and the time when the first results were ripe for publication. It is not claimed that the results here reported suffice for a perfectly satisfactory verification of the law referred to. They are, however, in the judgment of the authors an important step in that direction.

II. APPARATUS AND METHODS.

OUTLINE OF APPARATUS AND PLAN OF EXPERIMENTS.

The name " respiration calorimeter " is suggested by the fact that it is essentially a respiration apparatus with appliances for calorimetric measurements. As a respiration apparatus it is similar in principle to that of Pettenkofer. As an instrument for measuring heat it is a constant temperature calorimeter, the heat being absorbed and carried away by a current of water as rapidly as it is generated in the chamber. The arrangements for the measurement of both the respiratory products and the heat given off from the body differ in important respects from those of any other apparatus with which we are familiar.

The essential features of the apparatus are:

1. A chamber in which the subject of the experiment lives, eats, drinks, sleeps and works during a period of several days and nights. The chamber is furnished with a folding chair, table and bed.

2. Arrangements for ventilation by a current of air which is drawn from out of doors, and passes through the chamber. The volume of this air current is measured and its temperature is so regulated as to be the same on entering the chamber as upon leaving. Samples for analysis taken before it enters and after it leaves the chamber give data showing the amount of carbon dioxide and water given off from the body through the lungs and skin.

3. Arrangements for passing food and drink into the chamber and removing the solid and liquid excreta. These materials are analyzed and give data for calculating the income and outgo of nitrogen; and also of sulphur, phosphorus, chlorine and metallic elements if these are determined. Taken in connection with the determinations of carbon dioxide and water in the respiratory products, they show the income and outgo of carbon and hydrogen. The analysis of the food, and solid and liquid excreta enable us also to determine the so-called digestibility of the food, *i. e.*, the proportions of nutrients actually made available.

4. Arrangements for measuring the heat given off from the body of the man in the chamber and the heat equivalent of the external, muscular work. The heat given off is carried away by a current ot

cold water which passes through a series of pipes inside the chamber. By regulating the temperature of this water current as it enters, and also its rate of flow, it is possible to carry away the heat just as fast as it is generated, and thus maintain a constant temperature inside the chamber. The mass of the water and its increase of temperature are measured, thus determining the quantity of heat carried away.

In order that the heat taken up by the absorbers and carried out by the water current shall represent exactly the amount given off from the man's body or otherwise produced in the chamber, it is necessary to provide that there shall be no passage of heat through the walls, or rather that the small quantities that may pass in and out shall exactly counterbalance each other. Furthermore the ventilating current of air should leave the chamber at the same temperature as it enters, so that it shall carry out neither more nor less heat than it brings in, except that due to the greater quantity of water vapor in the air leaving the chamber over that in the air entering. This excess of water vapor is derived from the body of the subject, and the heat required to vaporize it must be added to the heat carried away by the current of water to obtain a measure of the total heat given off by the subject.

In the actual experiments the man remains for several days and nights (generally four days and five nights) in the chamber. The food and drink are passed in and the feces and urine removed at regular intervals. The diet is decided by the special question which is being studied, but is made agreeable to the subject of the experiment.

In some of the experiments the man had as little muscular exercise as practicable. In these so-called "rest experiments" no attempt was made to measure the muscular work. In others, so-called "work experiments," the amount of work done was measured by a specially devised ergometer, consisting of a stationary bicycle which was belted to a small dynamo. The electric current generated passed through an incandescent lamp inside the chamber where its energy was transformed into heat, the strength of the current and its voltage being determined by instruments outside the calorimeter. The heat derived from the muscular work done was thus added to, and measured with, the heat given off from the body. The duration of the work and the amount of the electric energy generated gave data for the computation of the amount of muscular work performed.

The men who have served as subjects of the experiments have not found their sojourn in the chamber either inconvenient or disagreeable. The experiments are, however, very laborious, and require not only the services of trained observers day and night for the chemical and physical determinations, but also a considerable amount of painstaking work in preparing the food and attending to the numerous details, before, during and after the experiment.

ARRANGEMENTS OF THE APPARATUS.

A general idea of the apparatus can be had from Fig. 1, taken from a photograph. This gives a general view of the principal parts of the apparatus, which stands in a basement room of the Orange Judd Hall of Natural Science, at Wesleyan University. In the center is the large chamber, which is surrounded by a sheathing of wood. At the end of the chamber, on the right, is shown a door, which serves also as a window. At the right of the window and just below it are the arrangements for cooling and measuring the current of water which brings away the heat from the interior of the chamber. At the left, in front of the large brick pillar, is a table at which an observer sits to record the temperature of the interior of the apparatus and of the currents of air and water, the temperature being measured electrically. Behind the brick pillar is an ammonia refrigerating machine, not shown in the picture. Its purpose is to cool the solution of calcium chlorid which is contained in a large tank in the center foreground. This tank (Fig. 11) is inclosed within a double-walled wooden casing. The liquid ammonia is evaporated in the coiled iron pipe, shown in Fig. 11. The ventilating current of air, before it enters the chamber, is passed through the copper condenser, *A*, which is immersed in the calcium chlorid solution in the right-hand corner of this tank, and thus cooled to a temperature of from $-19°$ to $-20°$ C. At this very low temperature nearly all of the water is removed from the air, so that it enters the chamber quite dry. Just before entering at the right of the

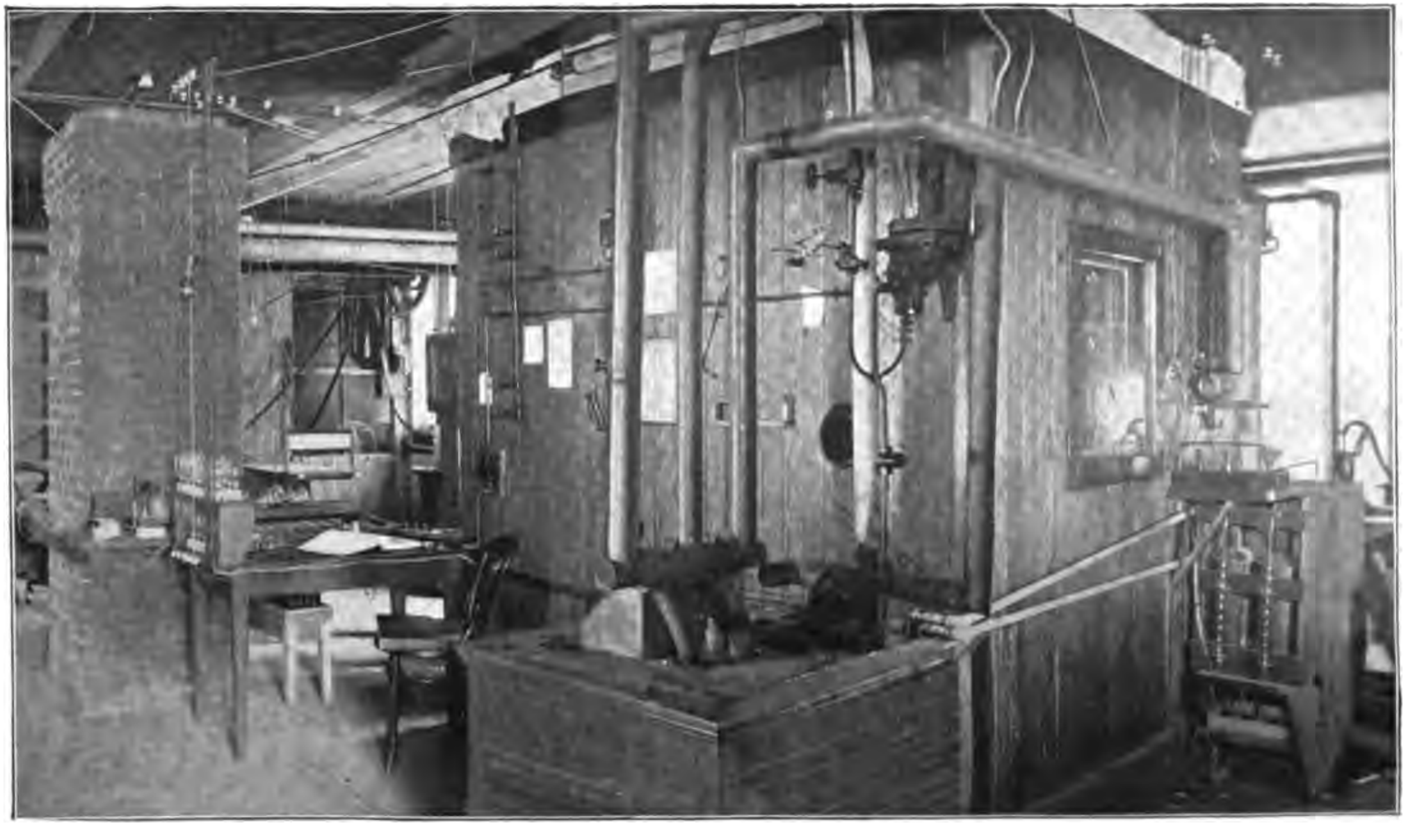

Fig. 1.

General view of the respiration calorimeter. Respiration chamber in center. Refrigerating tank in foreground. Water meter on the right. Observer's table on the left.

glass door it is warmed to the temperature of the interior of the chamber. On coming out it passes through a second condenser, consisting of a pair of copper cylinders, *B*, *C*, in the cold brine in the tank, and thus the larger part of the water which has been imparted to it by the respiration of the man inside the chamber is frozen and deposited. By means of valves contained in the valve boxes above the cylinders the air current may be diverted to a second pair of cooling cylinders, *D*, *E*. The first pair may then be removed and the deposited water determined by the increase in weight.

The window of the respiration chamber opens toward the east, so that the interior of the chamber is lighted from the two windows in the northeast corner of the room. At the west end of the room stands a 2-horsepower electric motor, belted to the driving pulley of a line shaft extending the entire length of the room. From this shaft power is taken to drive the ammonia refrigerating machine, the fans contained in the two fan boxes which circulate the air in the space surrounding the respiration chamber, and the meter pump which propels and measures the ventilating air current. A compression air pump is also driven from the shaft. This provides the compressed air which operates the valves of the meter pump. The air current passes from out of doors through a three-inch pipe into the refrigerating tank where the greater portion of its moisture is deposited, and thence to the respiration chamber (Fig. 5), after being warmed to the temperature of the latter. From the respiration chamber the air passes through the return pipe P_3 (Fig. 11) to the refrigerating tank, where its moisture is again deposited for the most part, and thence to the meter pump through P_4. Here the pump automatically records the volume of air and delivers every fiftieth cylinderful to the pans for analysis for carbon dioxid and residual moisture. Samples for analysis of the dried air of the pipes P_2, P_4 are drawn through quarter-inch brass pipes by the aspirators located at the south side of the room; and through similar pipes other samples are drawn off by aspirators in an adjoining room for duplicate analyses. The details of these operations will be given further on. On the north side of the pier near the middle of the room, and attached to the pier, is the galvanometer, used for measuring electrically temperatures and differences of temperature; and on the observer's table are located Wheatstone bridges, keys and resistances used in the measurement and regulation of temperatures, as hereafter described. Tables and other appliances afford facilities for part of the chemical work done in connection with the experiment, but most of the chemical work is done in other portions of the building.

ARRANGEMENTS FOR PREVENTING A GAIN OR LOSS OF HEAT THROUGH THE WALLS OF THE CALORIMETER.

1. Construction of the Calorimeter.

The construction of the calorimeter is shown in horizontal section in Fig. 2. The inner chamber is practically an apartment with double walls of metal. The inner of the two metal walls is of sheet copper, and incloses a space 2.15 meters (7 feet) long, 1.92 meters (6 feet 4 inches) high, and 1.22 meters (4 feet) wide, the corners being rounded. Copper was chosen because it can be obtained in large sheets, can take a high polish and so the more fully reflect radiant energy, is a good conductor of heat and thereby tends to equalize local differences of temperature, and has a rather small specific heat. A window in one end, 70 centimeters high by 49 centimeters wide, serves also the purpose of a door. During an experiment, however, the door is sealed and a circular opening, *E*, which has tightly fitting caps *a*, *b*, both inside and outside, is used for communication with the interior. Outside is a box, *c*, filled with non-conducting material to prevent the passage of heat through *E*. The seams are soldered and the chamber is air-tight when the door and other openings are sealed. The outer metal wall is of zinc, which is less expensive than copper, and serves the purpose sufficiently well. Between the two walls is an air space, *A*, of 7.6 centimeters (3 inches). In this stands a wooden framework to which the two metal walls are securely attached.

In order to protect the calorimeter from the fluctuations of temperature of the basement room in which it stands it is enclosed within three concentric walls of wood. Through each of these is an opening corresponding to the window in the wall of the calo-

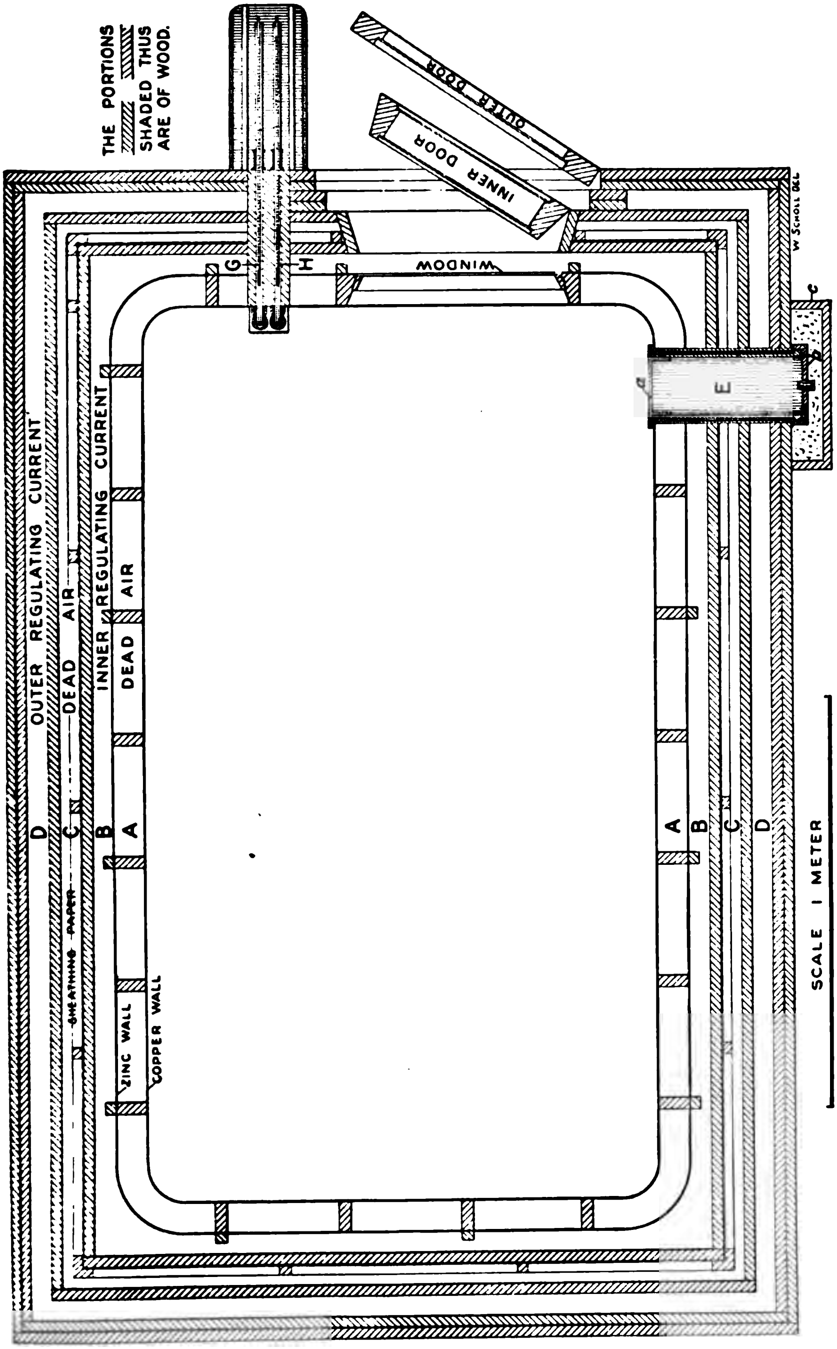

Fig. 2.
Horizontal section of the respiration calorimeter.

rimeter proper. These openings are closed by tightly fitting glass doors, hung on hinges.

Near these doors are shown the pipes through which the water enters and leaves the calorimeter. Its temperature on entering is measured by the mercury thermometer, *G*; and as it leaves, by a similar thermometer, *H*. Between the zinc wall and the innermost wooden wall is a 5-centimeter (2-inch) air space, *B*; between this wall and the next is a third air space, *C*, of 5 centimeters (2 inches); and, finally, between this wall and the outer one is a fourth air space, *D*, of 5 centimeters (2 inches). The wooden walls are made of matched pine, covered with building paper, and the outer one is double, with paper between. Of these four air spaces *A* and *C* are "dead" air spaces, while the air in the spaces *B* and *D* can be kept in constant circulation by means of rotary fans driven by power. Each of the air spaces *B* and *D* is continuous around the sides and over the top and bottom, and each communicates with its fan box in the rear by means of one passage extending from the top of the air space to the top of its fan box, and another from the bottom of the air space to the bottom of its fan box. Thus the air may be kept in constant circulation through these two air spaces and by heating or cooling this moving air the temperature of the corresponding space may be raised or lowered. We may also look upon the moving strata of air as shields, guarding the interior space occupied by the calorimeter from changes in temperature without. The walls at the top and bottom are constructed like those of the sides, but contain two flues for the regulating air currents. The air from the space *B* is drawn into the upper part of the bottom flue, and by means of the inner fan is driven into the lower part of the flue at the top. Openings along this flue allow the air to flow out over the top of the calorimeter, and thence down around the four sides to the bottom and so back to the fan again.

Similarly the air in the space *D* is kept in circulation by the fan in the outer fan box. The circulation in this space is in the opposite direction from that in *B*; that is, the air is taken from the top and driven in at the bottom. As the upper part of the room is usually much warmer than the floor, this arrangement for the outer circulating air tends to equalize the difference of temperature by drawing off the hot air from the top and driving it in at the bottom.

2. *The Thermo-electric Elements.*

In order to determine whether the inner or outer metal wall of the calorimeter is the warmer—that is, in order to know whether to heat or to cool the air space *B*—we use a large number of pairs (304) of thermo-electric junctions, distributed over the four sides, top and bottom of the calorimeter, one-half of the junctions (the iron-German-silver) being in close thermal contact with the copper wall and the other half (the German-silver-iron) with the zinc wall. One of these elements is shown in Fig. 3, where C_1 is a blank copper cartridge shell or cap, soldered to the copper wall, and maintaining the inclosed thermo-electric junction at the same temperature as the copper wall, and C_2 is a similar cap soldered to the zinc wall. There being copper leads connecting the several pairs, there are two junctions (copper-iron and German-silver-copper) in the outer cap; but these are thermo-electrically equivalent to the German-silver-iron junction. In reality there are four pairs of junctions in each group instead of a single pair as shown in the figure. These thermo-electric elements are made of silk-covered iron and German-

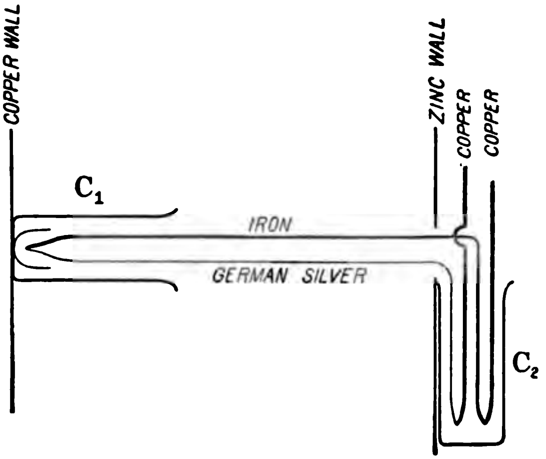

Fig. 3

Thermo-electric junctions. C_1 and C_2 are ordinary copper cartridge shells soldered to the copper and zinc walls respectively, and containing the junctions of the thermo-electric elements. Only one pair of junctions is shown in the figure, but there are really four such pairs in each group. The junctions are electrically insulated from the walls of the calorimeter, but in the best possible thermal contact.

silver wire No. 20. They are all in series, and are joined to a sensitive D'Arsonval galvanometer. The galvanometer is quick and deadbeat, and readings may be taken rapidly. If the copper wall is on the whole warmer than the zinc, the electromotive forces due to the junctions in contact with it overpower those due to the junctions in contact with the zinc wall and a current flows through the galvanometer, proportional in amount to the difference of temperature of the walls, and produces a deflection to the right. It is then necessary to warm the space *B*, which raises the temperature of the zinc wall, and the deflection will be reduced. If the deflection is to the left, the space *B* is cooled until the deflection is zero. A deflection of 1 division on the scale represents a difference in temperature between copper and zinc of about 0.007° C. An experienced observer can ordinarily keep the deflection within one or two divisions, and by taking frequent readings at regular intervals the + and — deflections are separately summed up and made to cancel each other as the experiment proceeds, and hence no "cooling correction" is necessary.

Of course the deflection of the galvanometer is due to the algebraic sum of all the electromotive forces at the 304 pairs of elements. At some places the copper may be warmer and at other places colder than the zinc, and the galvanometer indicates the average difference. In order that this average may be the more reliable the elements have been spaced as nearly uniformly as possible over the entire surface. There are 76 groups of elements of 4 pairs each: Of these 76 groups 11 are on the top, 11 on the bottom, 22 on the upper part of the four sides (over two-fifths of the whole area), and 32 on the lower part of the four sides (covering the remaining three-fifths of the area). Thus the elements are joined up in four sections, designated respectively as top, upper, lower, and bottom, and each section can be joined to the galvanometer separately. In practice, therefore, we can determine whether to heat or cool any of the four sections, top, upper, lower or bottom, in order that the deflection shall always be small for each section, and that the total deflection for all shall be zero. This secures a more perfect balance than could be had if only the total deflection were zero; for then an appreciable quantity of heat might be flowing in at the top and out at the bottom and the two quantities not exactly balancing each other.

There are 26 pairs of thermo-electric elements set in the wooden wall between the air spaces *B* and *D*, one set of junctions extending into the air space *B* and the other into *D*. These elements indicate the difference of temperature between *B* and *D*, and indicate when *D* must be heated or cooled to keep it nearly at the temperature of *B*.

3. *Heating and Cooling Appliances.*

In order to be able to heat or to cool any of the four sections of the air space *B* separately, a system of German-silver wires for heating and of iron pipes for cooling are arranged in this space, between the inner wooden wall and the zinc wall. German-silver wire No. 30 was stretched horizontally around the calorimeter, being supported and insulated by passing it through glass tubes set into vertical wooden strips which are attached to the zinc wall. The wire was divided into four sections corresponding to the four sections of the thermo-electric elements and the ends brought out so that an electric current could be passed through any of them at will, the strength of the current being regulated by variable resistances. In a similar manner ¼-inch (6-millimeter) iron pipes are arranged in four sections so that by opening a valve water from the city service can be passed through any of the pipes, to cool the surrounding space; when the water is shut off, a vent in each of the valves allows the pipes to drain. When only slight cooling is needed, the water may be allowed to flow only a few seconds at a time. In this manner the temperature of the air space can be perfectly controlled, and when the temperature within the calorimeter remains constant, as when heat is being generated at a uniform rate, the deflection can usually be kept within 1 division, which means an average difference of temperature between copper and zinc of less than 0.01°C.

Inasmuch as the temperature of the room is always lower near the floor than near the ceiling, the air circulating through the space *D* is warmed by passing over electric lamps or through a pipe heated by a Bunsen burner and made to enter at the bottom of the calorimeter. This tends to equalize the temperature between the top and the bottom, and guided by the indications of the second set of thermo-electric junctions, the air in *D* is kept nearly at the temperature of that in *B*. Thus the space *B* is perfectly protected from changes of temperature without, and it is possible to keep its temperature almost the same as that within the chamber.

4. *The Observer's Table.*

The terminals of the thermo-electric circuits and of the galvanometer are brought to the observer's table (Fig. 4). The galvanometer stands on a shelf attached to a brick pier, about 2 meters beyond the table, and the galvanometer scale is directly over the table, being also attached to the pier. A 4-point switch, *N*, joins the galvanometer to the several thermo-electric circuits in succession. If the key is on the first point, the thermo-electric circuit of the copper-zinc walls, designated as No. 1, is joined to the galvanometer, and the deflection indicates their difference in temperature, as already explained. An auxiliary switch, *M*, throws into the circuit any of the four sections separately, or all together when desired.

The 26 pairs of thermo-electric elements, designated as circuit No. 2, set in the wooden wall between the spaces *B* and *D*, are joined in series, and the terminals are brought out to the switch *N*, and joined to the galvanometer when the key is on the second contact. The galvanometer then indicates whether the space *D* is warmer or colder than *B*, and thus shows whether to heat or to cool the air flowing through it. If the temperature of the room is not very different from that of the calorimeter it is not necessary to heat or cool the air in *D*. But if the room is much colder than the respiration chamber, as is often the case at night when there is no steam in the radiators in the room, then by heating the air of *D* before it enters at the bottom it protects the space *B* from outside influence. The temperature of the room not infrequently falls in winter to 15° or 13°C., while that of the respiration chamber is maintained constant, usually at about 22°C.

When the key is on the third contact the galvanometer is short-circuited, and when on the fourth the thermo-electric junctions, designated as circuit No. 4, in the ventilating air current, hereafter to be described, are joined to the galvanometer.

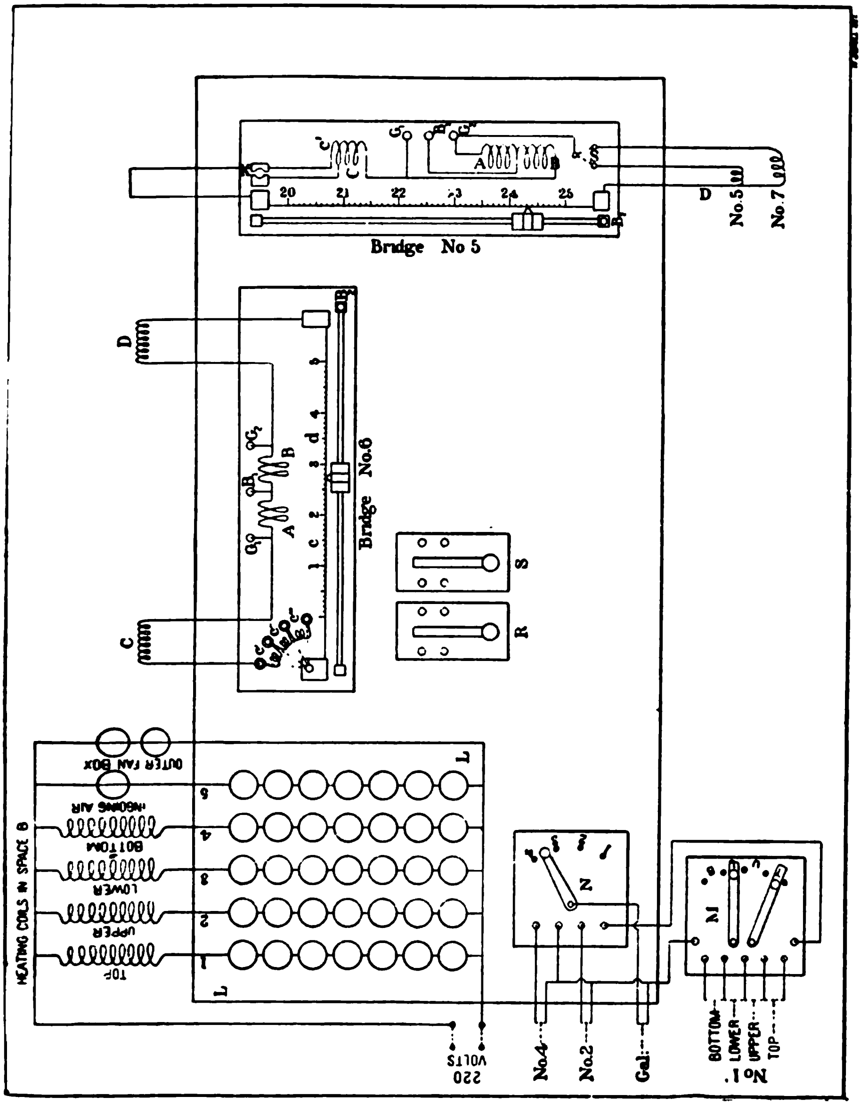

Fig. 4.

Diagram of electric apparatus at the observer's table. Bridge No. 5 is a special Wheatstone bridge used in measuring the temperature of the respiration chamber. Bridge No. 6 is used to measure the difference of temperature of the ingoing and outcoming water. *R*, *S* are two electric keys. *L*, *L* is a bank of incandescent electric lamps used as variable resistance. *M*, *N* are two multiple switches.

A bank of incandescent lamps, *LL*, is divided into five groups of 7 lamps each. Each of four groups is in series with one section of the German-silver heating coils already referred to, and which are wound around the calorimeter in the air space *B*. For example, the first section is in series with that section of the coil distributed over the top of the calorimeter, and the current is regulated by varying the number of lamps in series. Any one or more lamps can be cut out of circuit by a short-circuiting key. The lamps have different resistances, and by suitable combinations any desired current may be obtained, the source of current being the 220-volt-power circuit from which the electric motor is driven.

The fifth section of lamps is in series with an incandescent lamp placed in the pipe through which the ventilating current of air enters the respiration chamber, and by regulating the current through it, the temperature of the air as it enters is maintained the same as that of the air leaving.

Bridge No. 6 is a Wheatstone bridge, by means of which the difference of temperature of the stream of water as it enters and leaves the calorimeter is measured, and bridge No. 5 is a similar Wheatstone bridge for determining the temperature of the respiration chamber. These bridges will be described later.

Apparatus and Method for Measuring the Heat.

1. *The Water System.*

Having arranged the calorimeter so that no heat can escape or enter through its walls, it remains to carry away and measure the heat generated. This is done by a stream of water flowing through a copper "absorber" (*HH*, Fig. 5). A copper pipe of 6 millimeters (¼ inch) bore was bent into a rectangle 90 centimeters by 182 centimeters (3 feet by 6 feet). In order to increase the absorbing area of the pipe, one thousand disks, *II*, 5 centimeters (2 inches) in diameter and with a 1 centimeter (⅜ inch) hole in the center, stamped out of sheet copper, were slipped over the pipe like beads on a string, and soldered 6 millimeters (¼ inch) apart. The pipe and disks were then painted black. This absorber is suspended about 25 centimeters (10 inches) below the ceiling of the calorimeter, and 15 centimeters (6 inches) from the sides and ends. Water enters the calorimeter from the coolers through a pipe, *E* (Figs. 5 and 6), containing a mercury thermometer, *G*, and after flowing through the absorber leaves through a second pipe, *F*, near to and parallel with the first, containing a second thermometer, *H*. These two water pipes pass through holes in a cylindrical block of wood. The bulbs of the thermometers are at points, *g*, *h*, midway between

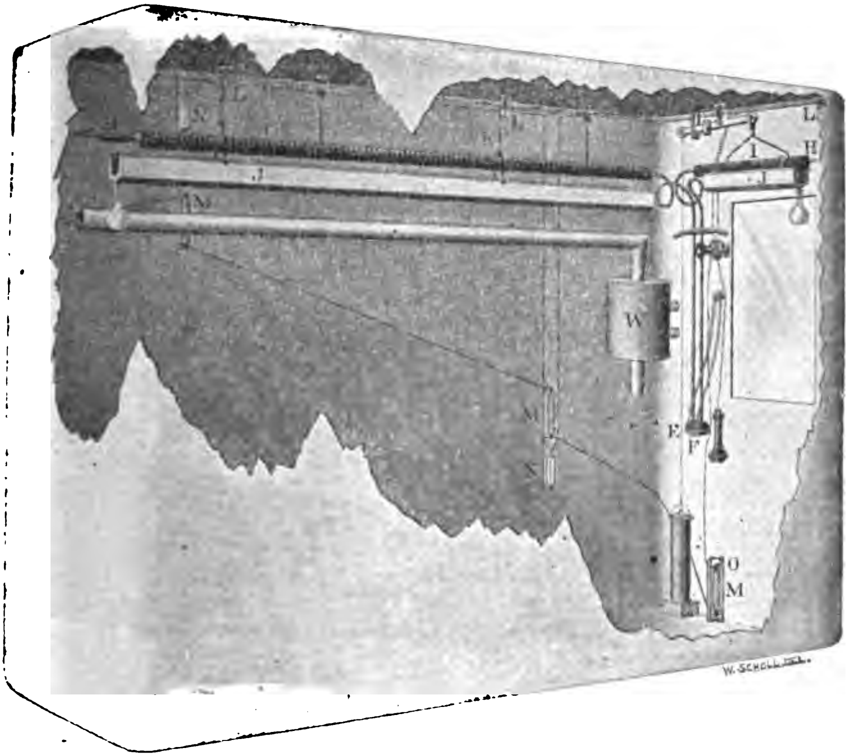

Fig. 5.

Interior of respiration chamber. This is the view which would be secured by breaking away a portion of one side and end of the interior copper wall of the respiration chamber. The ventilating air enters the chamber through the bottom of *W*, leaving from the rear of the chamber through the long tube connected with the upper end of *W*. The copper tubes *H*, *H*, strung with copper disks, *I*, *I*, constitute the absorbers through which flows the water current which carries away the heat generated in the chamber. *J*, *J* are the copper troughs for collecting the drip from the absorbers. *M*, *M*, *M* are the electrical thermometers which show the temperature of the air in the chamber; *N*, *N*, those which show the temperature of the copper wall.

the zinc and the copper walls. Hence one gives the temperature of the water just as it enters the respiration chamber and the other its temperature as it leaves. The thermometers are 60 centimeters (24 inches) in length, and are graduated from 0° to 30° C., in twentieths of a degree, and read to hundredths of a degree. They have been carefully compared with our standards. These thermometers are read by microscopes mounted as shown in Fig. 6, the thread of mercury bearing illuminated by small electric lamps. Their difference of temperature gives a measure of the heat absorbed within the calorimeter per gram of water.

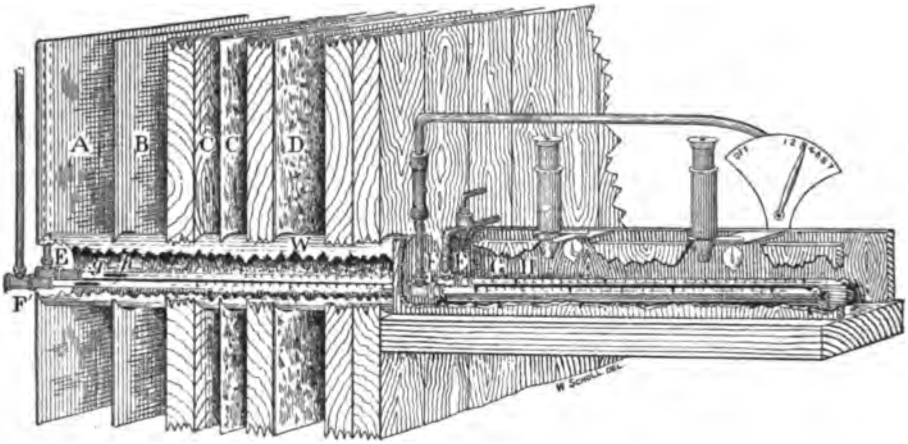

Fig. 6.

Section through walls of the calorimeter showing thermometers. *A* is the copper and *B* the zinc wall of the calorimeter chamber. *C* and *D* are the inclosing walls of wood and paper. The long thermometers *G*, *H*, extend into the two pipes *E*, *F*, through which the current of water enters and leaves the calorimeter. Readings are taken by means of the microscopes, the scale being illuminated by small electric lamps. The rate of flow of water is regulated by a valve with an index moving over a graduated scale.

In order to regulate the temperature of the ingoing water, and especially to have it as low as required, it is passed through a cooler. This consists of a pair of closed copper cylinders submerged in a bath of cold calcium chlorid. The temperature of this bath is kept at the desired point by pumping in cold calcium chlorid solution from the large refrigerating tank as often as necessary. The water to be cooled enters the first cooler, being carried to the bottom where the warm water prevents the formation of ice. This water being partially cooled enters the second cooler, being carried to the bottom as in the first one. Here it is cooled still further before going to the absorbers. By this means the ingoing water can be brought as low as 2° C. if desired, and any higher temperature can be obtained either by varying the temperature of the bath or by mixing the cold water with warmer taken directly from the supply. At the top of the room is a tank which is kept full and overflowing by a stream of water from the city service. From this tank is taken the water which flows through the absorbers. Having thus a constant head, the rate of flow of water through the absorbing pipes is uniform. A regulating valve with a pointer moving over a graduated arc (Fig. 6) enables one to vary the rate of flow within wide limits.

2. *The Water Meter.*

The water is received in a special form of water meter (Fig. 7), having a capacity of 10 kilograms. The water which has flowed through the absorbers runs into the small cup, *A*, which is divided by a vertical partition through the center and reaching from the bottom about half-way to the top. On either side of this partition are small tubes leading from the bottom of the cup, *A*, to the cylinders, *B* and *C*. When the water running into *A* falls on one side of the partition it will run down the tube to the corresponding cylinder, which will be filling up. As the level of the water rises it will finally be as high as the top of the partition in the little cup, *A*, when no more water will enter, but will overflow and run into the other cylinder. The first cylinder now contains just 10 kilograms of water.

In the narrow neck of the cylinder is a float, *F*, in the form of a test tube. This is hung from the end of a spring, and as the rising water lifts the float the spring comes in contact with the point *H*, and closes the circuit of an electric alarm bell which continues to ring until the siphon, *S*, is started and the cylinder begins to empty. After the siphon is started it will draw all the water from the cylinder down to a certain level without any further attention. The stream of water is diverted to the other side of the cup, *A*, by the operator before the siphon is started. Record is made at the instant the meter is filled and the interval between successive records

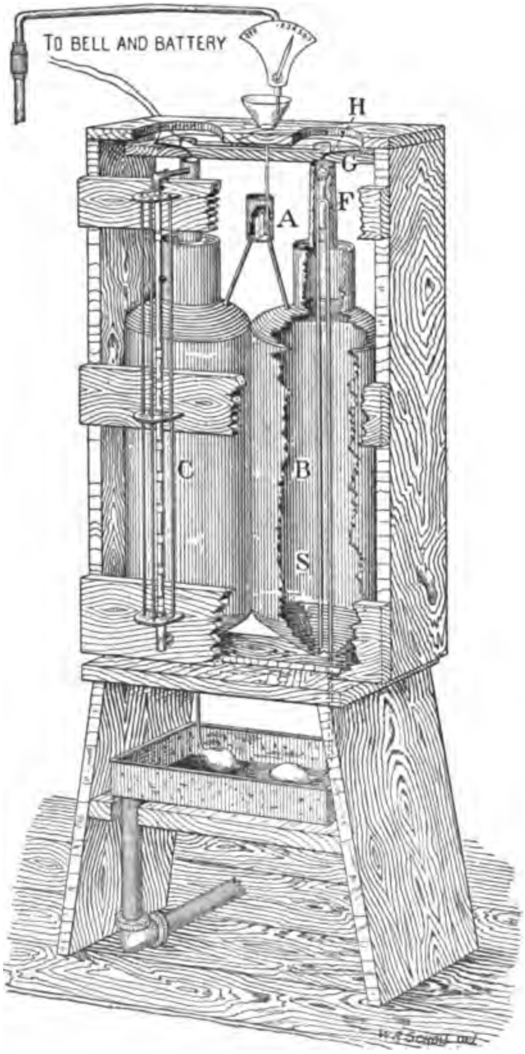

Fig. 7.

Water meter. The stream of water which carries away the heat generated in the respiration chamber flows into one of the cylinders *B*, *C*, until it is filled, when the stream is diverted automatically by the apparatus *A*, to the other cylinder. The first is emptied by means of the siphon *S*. The siphon is operated by the attendant whose attention is called by an electric bell which begins to ring as soon as the cylinder is filled, and continues ringing until the siphon is started.

gives the period for the corresponding 10 kilograms of water. Thus the product of the average difference of temperature between the ingoing and outgoing water by 10 gives the number of calories of heat brought away during the given period.

3. *Determining the Increase of Temperature of the Water.*

As a check upon the thermometer readings, and in order to avoid too frequent readings of the thermometers, two coils of copper wire are sealed in thin copper tubes and inserted in the water pipes near the bulbs of the thermometers. . They are joined to two comparison coils on the slide-wire Wheatstone bridge, No. 6 (Fig. 4), graduated so that readings on the scale give differences of temperature of the water in the pipes, 1 millimeter being 0.01° C. Readings on this bridge are usually taken every two minutes during an experiment, and careful readings on the mercury thermometers are taken occasionally for calibration purposes. This bridge is equivalent to what is generally known as a platinum thermometer, but inasmuch as copper coils are used instead of platinum, it is here called a "copper thermometer." The resistance of copper increases with its temperature, and hence the difference of temperature of two coils of wire may be accurately measured by determining their resistances.

The principle employed for the measurement of these resistances is, as just stated, that of the Wheatstone bridge, illustrated in Fig. 8. The four arms are A, B, $C + c$, $D + d$. The battery is joinedat B_1 and B_2, the galvanometer at G_1 and G_2. If A and B represent the proportional arms of the bridge, which are here equal, C and D the two copper coils in the water

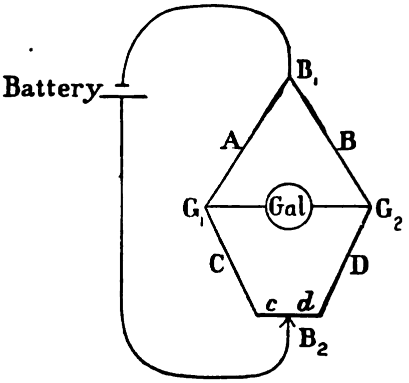

Fig. 8.

Diagram illustrating use of copper thermometer. This is a Wheatstone bridge in which A and B are two equal comparison coils; C is a fixed resistance; D is the resistance coil in the calorimeter, and c and d are the two parts of the straight wire on the bridges shown in Plate VI.

pipes, and $c + d$ the slide wire of the bridge, then for no current through the galvanometer the following proportions must be true :

$$A : B : : C + c : D + d.$$

Suppose now the coil D is warmed so that its resistance is increased. To keep the bridge still balanced, B_2 must be moved along the slide-wire reducing d, and, at the same time, increasing c, until the bridge is again balanced. By properly graduating the slide-wire scale the position of B_2 will show directly the difference in temperature between the coils C and D; and these coils of fine insulated copper wire, inclosed in very thin copper tubes, quickly take up the temperature of the water by which they are surrounded.

Referring now to Fig. 4, the connection of bridge No. 6 will be readily seen. The parts are lettered the same as in Fig. 8. In addition are the small coils c', c'', c'''. The length of the bridge wire corresponds to a difference of temperature of 5° in the coils C, D. If the difference increases above this, the coil c' is added to C, so that the slider instead of being at " 5 " will be at " 0," and then differences of temperature between 5° C. and 10° C. can be measured. In the same way, when the difference reaches 10° C., c'' is added to $C + c'$, and "0" then corresponds to 10° C. The connections are made and changed by means of a heavy link dipping into mercury cups. The scale is 500 millimeters in length, and hence the position of the slider can easily be read off in hundredths of a degree.

The rate at which heat is taken up by the absorbers depends upon their temperature as compared with that of the surrounding space, and the freedom of circulation of air around them. Three methods are employed to diminish this rate below the maximum. First, the water may be allowed to enter at a higher temperature than the minimum of 2° C.; second, the water may be made to flow slowly through the absorbers so that its average temperature is higher ; and third, shields may be put around the absorbers. Deep copper troughs, *J*, *J*, hang under the absorbers (see Fig. 5), one on each side and one on each end. The side troughs or shields can be operated from without by means of cords passing over pulleys, *L*, *L*, which are wound up by rods reaching through the side of the calorimeter. Raising a shield around a portion of the absorber impedes the circulation of the air, the colder air within the shield being able to escape only by rising and flowing over the sides. As these shields can be raised to any desired point, and the temperature and rate of flow of the entering water can be regulated at will, any desired rate of absorption from 1 to 250 calories per hour, its maximum rate, can be realized.

4. *Determining the Temperature of the Calorimeter.*

In order to measure not only the total amount of heat evolved by the subject, but also the rate at any and all times, it is necessary to keep the interior temperature of the calorimeter very nearly constant, and also to measure the small changes in temperature which may occur. For this purpose we have mounted six coils of No. 32 copper wire near the walls of the chamber, connected them in series with one another and joined them to a slide-wire bridge (No. 5, Fig. 4), outside, so graduated as to read off the temperatures directly in degrees Centigrade, 1° extending over a hundred millimeters on the scale. Thus the temperature is read to the hundredth of a degree. This bridge is similar to No. 6. *A* and *B* are the proportional arms of the bridge, here equal to each other, *C* is the comparison coil mounted on the bridge. The wire of coil *C* (as well as both bridge wires) was kindly furnished by the Weston Electric Instrument Company, and has a zero temperature coefficient, so that its resistance is constant. Hence the position of the slider is changed only when the temperature of the copper thermometers, *D*, varies. At *K* is a switch for short circuiting the small coil *c'*. When this is closed the scale reads from 17° C. to 22° C.; when open, from 20° C. to 25° C. The copper coil *D*, which forms the fourth arm of the Wheatstone Bridge, is subdivided into six separate coils. Two of these are mounted on each side of the chamber and one on each end, each being at a different height, *M*, *M*, *M* (Fig. 5). Thus they indicate a fair average temperature. These coils are wound on light wooden frames and are covered by a perforated brass tube. They are thoroughly insulated from the copper wall, and further protected from injury by guards, *O* (Fig. 5). They are very sensitive to slight changes in the temperature of the air.

A second set of similar coils is enclosed in copper boxes and attached in close thermal contact to the copper walls. They may be joined to the same bridge by a key, *S*, and indicate the temperature of the copper walls. These copper coils, with their slide-wire bridge, we call the "copper thermometers." A mercury thermometer passes through the wooden and zinc walls, and its bulb lies in a copper pocket which is soldered to the copper wall. This gives the temperature of the copper at that place, and its readings are compared with the "copper thermometer" readings.

The Ventilating Air Current.

1. *Measuring and Regulating the Temperature of the Ingoing Air.*

The temperature of the out-coming air will be the same as that inside the chamber. If the in-going air can be kept at the same

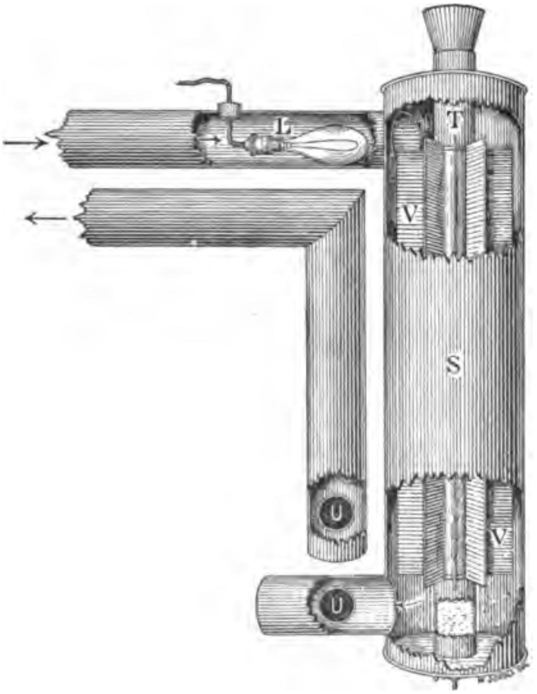

Fig. 9.

Air temperature regulator. The ventilating current of air passes through this regulator on its way to the calorimeter and the temperature is raised by the lamp, *L*, or lowered by water contained in the cylinder, *T*, as may be necessary to make it the same as the temperature of the air escaping through the exit pipe, *U*.

temperature there will be no gain or loss of heat due to the circulation of the air.

The in-going air passes through the "regulator" before entering the chamber. This regulator, shown in Fig. 9, is a copper cylinder 76 centimeters (30 inches) long and 18 centimeters (7 inches) in diameter. Within this is another cylinder 5 centimeters (2 inches) in diameter extending through the ends of the outer one. On the outside of this small cylinder are soldered eight vanes, *V*, which reach nearly to the outside wall, thus increasing the effective area of the cylinder, which is filled with water to increase its capacity for heat. The air comes to the regulator a few degrees too cold, and it is warmed by an incandescent lamp *L*, placed in the pipe. The current in the lamp is varied to give the required amount of heat. After passing over the lamp and being warmed to about the desired temperature the air enters the regulator *S*, where it attains its final uniform temperature. If it enters a little warmer it will impart its surplus heat to the water in the small cylinder, *T*, while if it enters

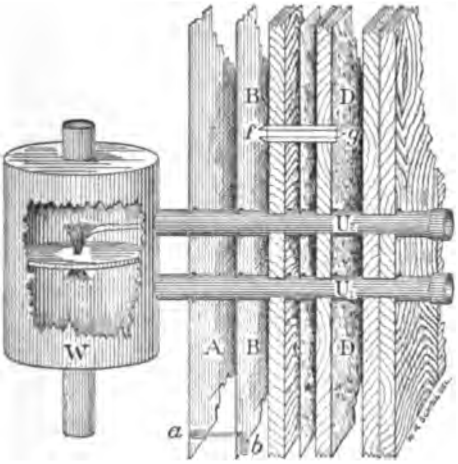

Fig. 10.

"Vestibule." The object of this apparatus is to facilitate the comparing of the temperature of the in-coming and out-going ventilating current of air. The air enters the lower part of the vestibule through the lower pipe U'_1, and leaves the upper part through U'_2. The thermo-electric junctions set in the partition between the upper and lower parts of the vestibule indicate any difference of temperature between the entering air and that which is about to flow out.

slightly cooler it will receive heat from the same source. Thus it will enter the chamber through the tube, U, at a nearly constant temperature. This regulator of large capacity for heat steadies the temperature of the air current as a massive fly wheel of an engine steadies its speed.

Within the chamber is the "vestibule" (Fig. 10), through which pass the currents of in-going and out-coming air. This is a double-walled copper chamber, divided into two portions by a double-walled partition. The out-coming air enters the top and passes out through the tube, U_2. This space will then have the temperature of the out-coming air. In the same way the in-going air enters through the tube, U_1, and leaves at the bottom. The lower space is then at the temperature of the in-going air. Extending through the partition on either side are twenty-four pairs of iron-German-silver thermo-electric couples. These are connected to the galvanometer through point 4 of switch N (Fig. 4), and if there is any difference of temperature between the air in the two spaces it will cause a deflection of the galvanometer. If this shows that the in-going air is cooler than the outcoming, the current in the lamp (Fig. 9) is increased. If the air should require to be cooled, as rarely occurs, this may be effected by drawing off a portion of the water and replacing it by colder water.

2. *Refrigerating Apparatus.*

The refrigerating tank is shown in Fig. 11. Liquid ammonia is supplied to the tank through the iron pipe marked P_5 from an ammonia machine in another part of the room. This liquid ammonia, initially at high pressure, passes through a reducing valve and flows into the coil of iron pipe shown in the figure. Here it evaporates, as it flows around the coil, as rapidly as heat can be absorbed from the solution of calcium chlorid which fills the tank. The temperature of this solution is thus reduced to about $-18°$ or $-20°$C. The coil of pipe through which the ammonia passes consists of 88 meters (287 feet) of 2.5 centimeters (1 inch) extra-heavy pipe electrically welded so as to form one continuous tube. There are two sets of air refrigerators or "freezers" in the tank. The first is marked A, and is made of sheet copper. It consists practically of two large **U** tubes of copper immersed in the brine and connected by horizontal elbows. Copper vanes attached to the walls of the freezer increase the cooling surface. The air enters the pipe P_1 from

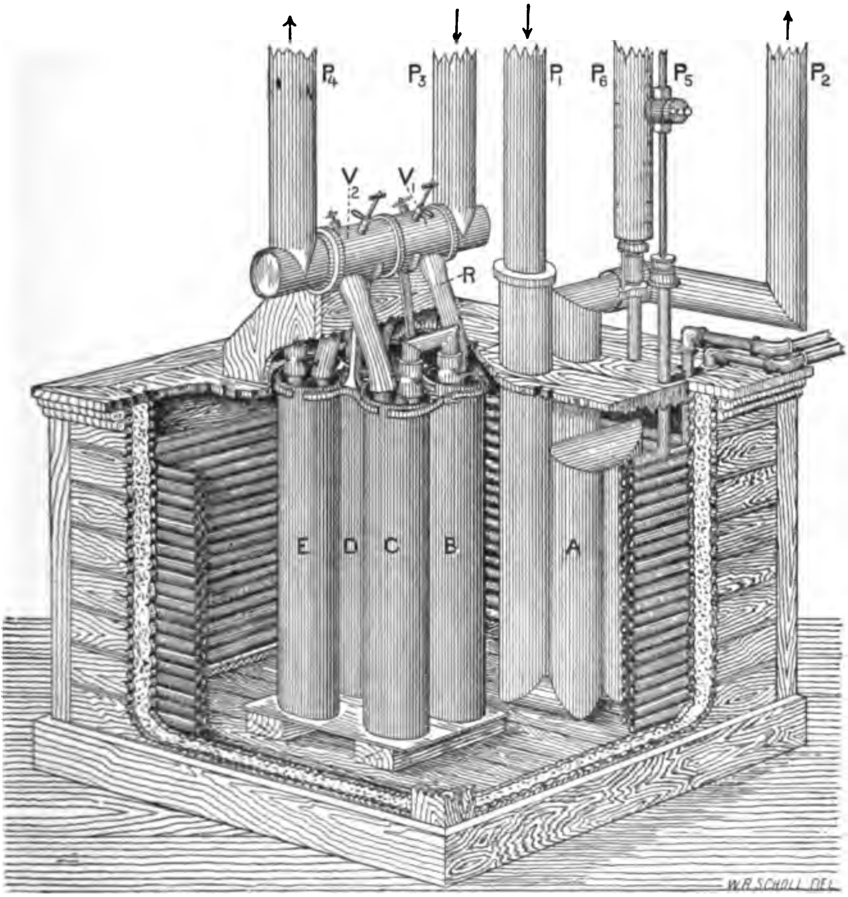

Fig. 11.

Refrigerating apparatus. The chief object of this is to remove the greater portion of the moisture from the ventilating current of air before it enters and after it leaves the respiration chamber. For this purpose the air is passed through copper cylinders, "coolers," *A*, *E*, immersed in brine (of calcium chlorid) which is cooled to a temperature of — 18° to — 20° C. by aid of ammonia expanding in the coiled pipe seen against the sides of the tank in the figure. The air flows through the cooler, *A*, on its way to the respiration chamber, and through *B*, and *C* or *D* and *E* on its way from the chamber to the pumps. The valves, V_1 and V_2, are used to direct the outcoming air current from one pair of cylinders to the other.

out-of-doors laden with more or less moisture. Reaching the freezer *A* its temperature is quickly lowered nearly to that of the surrounding brine and nearly all of its moisture is deposited upon the inner walls of the freezer. The air leaving the freezer *A*, passes along the pipe P_2 to the calorimeter, being warmed to the temperature of the room before reaching the chamber. Its moisture content as it enters the calorimeter is quite small and uniform.

Returning from the calorimeter laden with water vapor, the air passes through the second system of refrigerators or freezers, in which the greater portion of this moisture is condensed. But in order to be able to collect and measure the deposited moisture for any given period, this system of freezers is differently constructed from that used for the in-coming air. Four cylindrical brass vessels are clamped together, as shown in the figure, and fitting closely inside them are four cylinders or freezers made of sheet copper. Each of these is about 57 centimeters (22.5 inches) long and 12 centimeters (4.75 inches) in diameter, and is divided vertically by a partition which extends from the top nearly to the bottom. The air enters through a tube in the top, flows down to the bottom, and, rising on the other side of the partition, passes by means of a double-elbow connection to a second freezer, where the larger portion of the water not removed by the first freezer is condensed. In order to give a maximum cooling surface, vanes of sheet copper are soldered to the interior surface of the cylinders extending radially to within a short distance of the middle partition. The space between the inner cylinder or freezer and the outer protecting jacket is filled with alcohol, in order to insure a freer passage of heat from the air current to the cooling brine. The freezers are removed and weighed at intervals, generally of six hours. In order that this may be done conveniently and without interfering with the regular flow of the air current two pairs of cylinders are used and a valve box is so arranged that the air current can be easily diverted from one pair to the other. This consists of two pieces of brass pipe, 10 centimeters (4 inches) in diameter, placed end to end. The adjoining ends are separated by a disk of vulcanized fiber, which serves to prevent the passage of both air and heat from one tube to the other, while the open ends are connected to the air pipes P_3 and P_4 (Fig.

11). The valve box is provided with four lateral apertures, fitted with collars and rubber couplings, to connect it with the freezers. An arrangement is made for conveniently opening and closing these apertures by rubber stoppers, which are held by brass rods reaching through the walls of the valve box and worked from the outside. Two openings on one side are connected with one pair of freezers, while the two on the other side are closed. When it is desired to change the freezers the current is diverted from one pair to the other by means of these valves. The first pair of freezers is then taken out, warmed, dried and weighed; the increase in weight during the period gives the moisture condensed from the air current.

3. *The Blakeslee Meter Pump.*

Two forms of apparatus have been used for maintaining the air current and measuring its volume. The first consisted of an exhausting air pump used in connection with a special form of gas meter made by Elster in Berlin. With this we did a large amount of preliminary work, but were unable to make measurements of air volumes as accurately as we desired.

Aspirators containing 150 liters each were employed at the outset and are still used for taking samples of air for analysis. Measurements with these aspirators have been found quite accurate. But a much more satisfactory arrangement, and one which serves the threefold purpose of maintaining the air current, measuring its volume, and delivering aliquot samples of convenient volumes for analyses is an apparatus designed and constructed by Mr. O. S. Blakeslee and appropriately designated by him as a "meter pump." This is shown in Fig. 12. The essential parts for maintaining the air current and measuring its volume are cylinders of steel. There are two pumps, which work synchronously. Three steel cylinders are employed for each pump. Two of them are fixed concentrically, with a narrow annular space between them. This space is nearly filled with mercury and in it plays the third cylinder, its lower portion being immersed in the mercury. This moving cylinder is closed at the top and is raised and lowered by a walking beam. The length of the stroke of the cylinder is uniform, and the volume of air delivered at each stroke is constant. The temperature of the pump is kept practically constant, so that the mass of air of each stroke is constant. Like the moving cylinder, the inner cylinder is also covered at the top, but through this cover are two circular apertures, opened and closed alternately by an automatic valve.

The pump is driven by a belt on the large pulley, shown back of the walking beam, from a countershaft overhead. The speed is reduced by gearing, so that the face wheel in front rotates about ten times a minute. On the same shaft with the walking beam is a sleeve with a projecting arm, which is nearly hidden behind a portion of the walking beam. The end of this arm is attached to the face wheel by a connecting rod, as shown in the figure. This conveys an oscillatory motion to the sleeve, which is transmitted to the walking beam by the straight steel springs, the ends of which are connected about the middle of the arcs.

The moving cylinders are suspended from the circular ends of the walking beam by flexible steel straps, each of which is wrapped around the end of the beam as the cylinder rises, as shown at the right in the figure. The length of the stroke is determined by stops on the rods at the sides of the pump. These rods are fastened at the upper ends to the moving cylinders. The cylinders are thus stopped before the connecting rod has reached the end of its stroke or "dead point," and therefore the sleeve continues to turn after the walking beam and cylinders have come to rest. This is made possible by the long steel springs which drive the walking beam, as they bend enough to allow the sleeve to turn a little after the walking beam has been stopped.

During this short interval, while the cylinders are at rest, the valves are reversed by means of compressed air, so that as either cylinder descends the air will flow into the exit pipe. As seen in the figure, there are two pipes running under each pump. These pipes extend up inside the pump to the top of the stationary cylinders, the ends just reaching through the bottom of the pump proper. The valve is so arranged that when one of these pipes is closed the other is open. In front of the standard carrying the driving mechanism of the pump is seen a small rod which extends below the base. This is driven from the sleeve by a pair of beveled gears, and hence it also has an oscillating movement. As soon as the

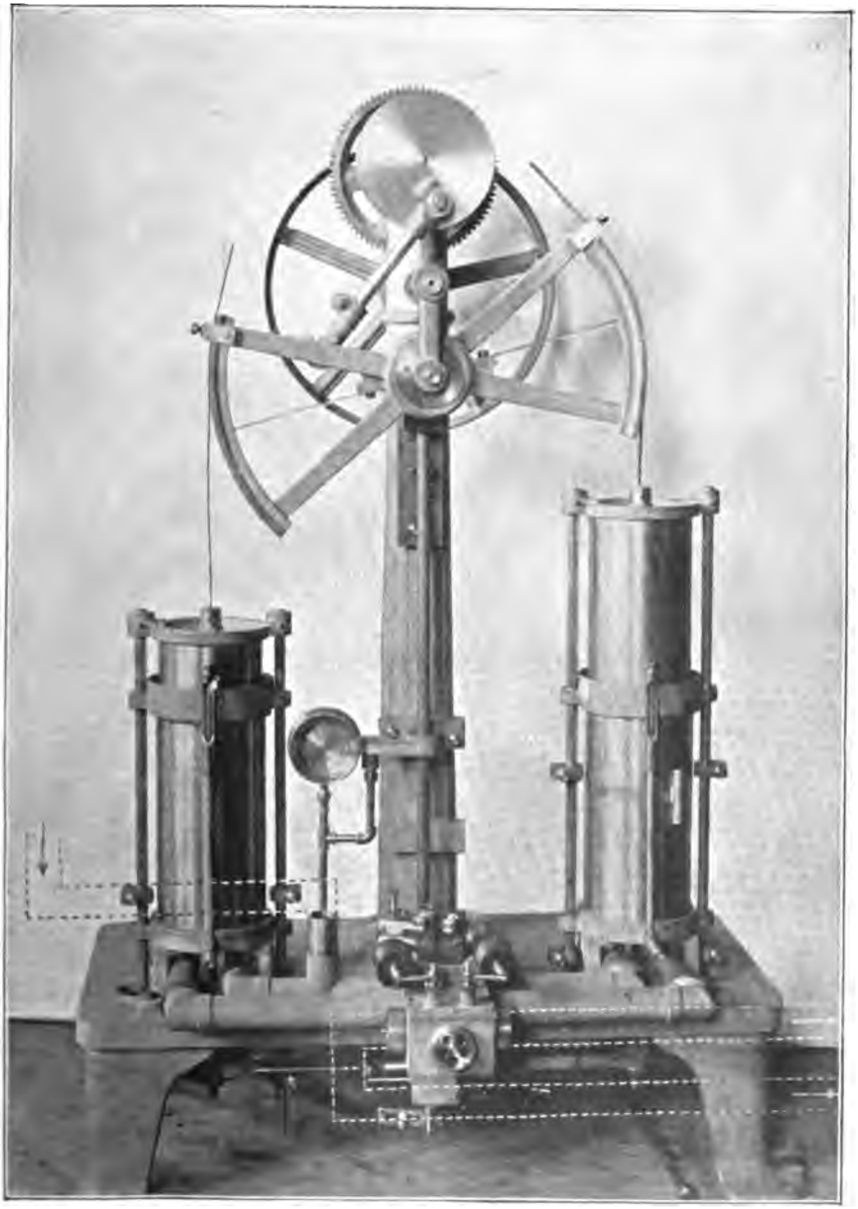

Fig. 12.

Blakeslee Meter Pump. The dotted lines show the positions of the pipes through which the main air current enters and the samples for analysis pass out.

cylinders have come to rest a port in the compressed air chamber is opened ; the air escapes into a cylinder ; the piston makes one stroke, which reverses the valves in both pumps, setting one so that air may be drawn in from the in-going pipe and the other so that the air may be expelled into the exit pipe. All this is done while the cylinders are at rest, and thus the air within them is at atmospheric pressure. The volume of each cylinder is known from its dimensions, and an automatic counter records the number of strokes.

In front of the base of the standard are seen the electro-magnets used in taking the samples for analysis. Below them is the chamber through which all of the air from the exit pipe must pass before it can escape. There are three openings from this chamber—one in front and one at either side. From the side openings are pipes (shown by the dotted lines) leading to the receptacles for holding the samples before analysis, termed briefly "pans." Likewise a pipe placed on the opening in front conducts the main current of air to the boxes surrounding the pans. This gives the same atmosphere both within and without the pans, and has been found by experiment to prevent a transfer of moisture through the sheet-rubber tops of the pans.

The method of taking the samples is as follows: Between the electro-magnets and the standard is a rachet wheel having 100 teeth. At each stroke of the pump this wheel is advanced by one tooth. Mounted on this wheel is a pin which closes an electric circuit once every revolution—that is, once every hundred strokes. The current flows through one of the electro-magnets, drawing in its plunger. This motion closes the aperture in front, at the same time opening the one at the side, and thus allowing the air from a complete double stroke (both cylinders full) to pass into the corresponding pan. The electric circuit is then broken, which releases the plunger of the magnet, and the air continues to flow through the opening in front. A second pin in the toothed wheel, on the opposite side from the first, closes a second circuit through the other electro-magnet, so that the intermediate fiftieth strokes are delivered into the other pan for duplicate analysis. This pump, although necessarily somewhat complex in construction is reliable and satisfactory in its operation.

4. *Analysis of the Samples of Air.*

The method of analysis of the air current consists in passing the sample through U tubes containing sulphuric acid for the removal of the water and soda-lime for the removal of the carbon dioxid.

These U tubes are about 12 centimeters (5 inches) in length and 1.5 centimeters (5/8 inch) in diameter. Those for the absorption of water are about two-thirds filled with pumice stone drenched with concentrated sulphuric acid. In use these tubes are laid nearly flat, with just enough incline to the tube to avoid all danger of the acid coming in contact with the rubber stoppers through which the small connecting glass tubes pass. Detailed experiments have shown that the absorption of water vapor from an air current flowing at a rate as high as 750 cubic centimeters per minute is complete by the use of one such sulphuric-acid tube until the acid has taken up 1 gram of water, after which complete absorption from a current flowing at this rate can not be depended upon.

While only one U tube is necessary for the complete absorption of the water, a system of three U tubes is used for the complete removal of the carbon dioxid from the sample. Two of these are filled with soda-lime for the absorption of the carbon dioxid itself; the third contains sulphuric acid, which serves for the collection of the water taken up from the soda-lime by the current of perfectly dry air passing over it. As the absorption of carbon dioxid takes place the yellowish color of the soda-lime gives place to the white color of the sodium and calcium carbonates that are formed, thus making it easy to see when a fresh tube is necessary. In the analysis of the incoming air, which contains very small quantities of carbon dioxid, one soda-lime tube, followed by a sulphuric-acid tube, is found sufficient, but for the outgoing air three tubes are used, as above described. When the soda-lime in one is nearly saturated the tube is removed, the second moved forward to take its place, and a fresh tube is added in place of the second. In this way the second tube in the system is always filled with fresh soda-lime. It has been found that two soda-lime tubes will remove all of the carbon dioxid from an air current flowing at a rate as high as 750 cubic centimeters per minute.

5. *Residual Carbon Dioxid and Water Vapor in the Respiration Chamber.*

The term "residual carbon dioxid and water vapor" is applied to the total amount of these materials remaining in the chamber at any given time.

In the check experiments described beyond, in which alcohol was burned, and in the experiments with man, the quantities of carbon dioxid and water vapor in the air of the chamber were found to be subject to considerable variations. In the experiments in which alcohol was burned in the chamber these fluctuations are due to the combustion of the alcohol at different rates; in experiments with a man they are due to differences in the amount of external or internal work done, and the consequent changes in the amount of material oxidized and the carbon dioxid and water eliminated.

In order therefore to determine the exact amounts of carbon dioxid and water given off during any given time from the combustion of alcohol in a lamp or of food in the body, it is necessary to know not only the amount of these respiration products in the ventilating air current during this period, but the variation of the residual amount in the chamber. For this purpose a sample consisting of 10 or 12 liters of air is drawn from the interior of the chamber through absorption tubes and the number of milligrams of carbon dioxid and water vapor per liter ascertained. This gives the total amounts of these products in the chamber the capacity of which is about 4800 liters, at the time the sample was drawn. Residual analyses are thus made at the beginning of an experiment and at the close of each experimental period.

The residual analyses therefore give data for determining a positive or negative correction which is applied to the amount of respiratory products found in the ventilating air current in order to obtain the quantities of these products actually given off during each experimental period.

(*To be continued.*)

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A METHOD FOR THE STUDY OF PHOSPHORESCENT SULPHIDES.

By Fred. E. Kester.

With such phosphorescent materials as are luminous for only a short time after illumination, Becquerel[1] used the phosphoroscope for photometric measurements. A large part of his quantitative study of phosphorescence was upon this kind of materials. To obtain measurements on substances, which continue to emit light for a number of minutes, and in some cases for several hours, after illumination, he made direct observations by adjusting the intensity of his comparison surface to equality with the transient intensity of the test substance and noting the instant at which the adjustment was made.

So far as I have been able to find, later observers, notably L. Darwin,[2] E. Wiedemann,[3] H. Becquerel[4] and C. Henry,[5] have used these two methods with slight modifications to suit special needs.

I. Apparatus.

In order to study the luminosity of sulphides of the alkaline earths between the limits imposed, on the one hand, by the intermittent character of the observations when the phosphoroscope is used, and on the other by the difficulty of making, by direct observation, a measurement of intensity of phosphorescence very soon after an illumination, I used the following device. A vertical cylinder of the test substance was rotated about its axis and was illuminated on one side through an aperture which was placed quite close to the cylinder. Then the emitted light was examined by a spectrometer, the slit of which was diametrically opposite the illuminating aperture. With a constant intensity of illuminating light and a constant speed of rotation, however slow, the intensity of the emitted light, as observed in the spectrometer, would, of course, remain the same, so that there would be no difficulties in making spectrophotometric comparisons, aside from those which necessarily accompany work with very weak intensities.

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[1] La Lumière, I, p. 244.

[2] Chem. News, 42, p. 302, 1880; Phil. Mag., V-11, p. 210, 1881.

[3] Wied. Annalen, 37, p. 177, 1889; Phil. Mag., V-28, p. 149, 1889; Eder's Jahrbuch f. Photographie, 5, p. 587.

[4] Comptes Rendus, 113, pp. 618-672, 1891.

[5] Comptes Rendus, 115, pp. 505-602, 1892.

It was my first intention to get the test substance into cylindrical form by filling a piece of glass tubing with it; this method would have left the phosphorescent powder in its natural form. But I was unable to find a piece of tubing which did not diffuse into the spectrometer some of the light carried around from the aperture by the internal reflections within the glass. The diffusion was caused by air bubbles and milky streaks inside the glass.

I finally decided to fix upon the convex surface of a turned brass cylinder, 27 mm. in diameter, a thin layer of the sulphide, by mixing a small quantity of it with a few drops of varnish and enough turpentine to make the mixture of the proper consistency, and then painting the convex surface with it. Varnish and turpentine were chosen for this purpose because neither of them has any tendency to reduce the sulphide to a sulphate by oxidation and because the varnish, when dry, protects the material from this oxidizing action of the moisture of the air. The fact that the powder, when so treated, was far from its natural state was a serious objection to this method.

The exciting light (from an arc lamp or from the sun) was formed into a spectrum and the cylinder with its aperture was placed in that part where the efficient wave-lengths lay. It was then necessary to measure the energy in the region of the spectrum which was used in exciting phosphorescence. The usual methods of the thermopile and bolometer, all of which necessitate the use of a delicate galvanometer, were not available because of magnetic disturbances; and a photometric method was not convenient. After a number of attempts to devise a suitable method, it was suggested to me that a Crook's radiometer be built in the form which Mr. E. F. Nichols[1] had used, in some measurements of radiations of long wave-length,

[1] Phys. Review, IV., 297, 1897.

while in Berlin. This was done, the piece being made very similar in form to his.

It may be of sufficient interest to mention here a difficulty which I encountered in the process of construction of the radiometer. In a tentative form of the apparatus a metal top, with a vertical brass tube soldered into it, was used; into the brass tube was sealed the tube of a glass stop-cock by means of ordinary sealing wax. A number of observers had had trouble with sealing wax when used in the joints of high vacuum pieces, but from all the statements noticed the trouble seemed to arise from leakage. This it was hoped to avoid by making between the glass and brass tubes (the brass being on the outside) a long, firm seal. However, in this form, the enclosure could not be maintained at a sufficiently low pressure, and a search for leaks was commenced. After some time it was noticed that the pressure did not rise steadily, but came up to a certain value and then hung there, indicating the presence of a vapor tension rather than of a leak. The sealing wax was suspected at once and in order to try it a few small pieces were placed in a clean vacuum tube which was then pumped to a high vacuum and sealed off. The pressure in the tube (indicated by the character of electric discharge through it) rose to one or two tenths of a millimeter in about half an hour and then remained constant.

A glass top was then ground to fit the metal body of the radiometer, and the tube of the glass stop-cock welded to it. Even in this final form the piece leaked a little, so that it was necessary to standardize it from day to day by means of a constant source of radiation. A Hefner photometric lamp was used for this purpose.

The arrangement of pieces was, then, as shown in Fig. 1. The aperture A in the wall of the dark room was illuminated from without by an arc lamp, with suitable condensing lens, or by sunlight, and the spectrum of the light brought to a focus with the blue end on the cylinder T. The mirror M had its upper edge on the same level with the lower edge of the aperture A, and was so placed as to reflect light, into the radiometer, of the same wave-length as that used in exciting phosphorescence, the spectrum being so adjusted as to cover both M and A.

The standard for photometric comparisons was a magnesium oxide surface illuminated by an acetylene flame which was furnished with gas at constant pressure. The light diffused from the surface reached the spectrometer through two Nicol prisms, *P* and *A*, and a total reflection prism which covered the lower half of the collimator slit. The upper half of this slit received light from the phosphorescent cylinder.

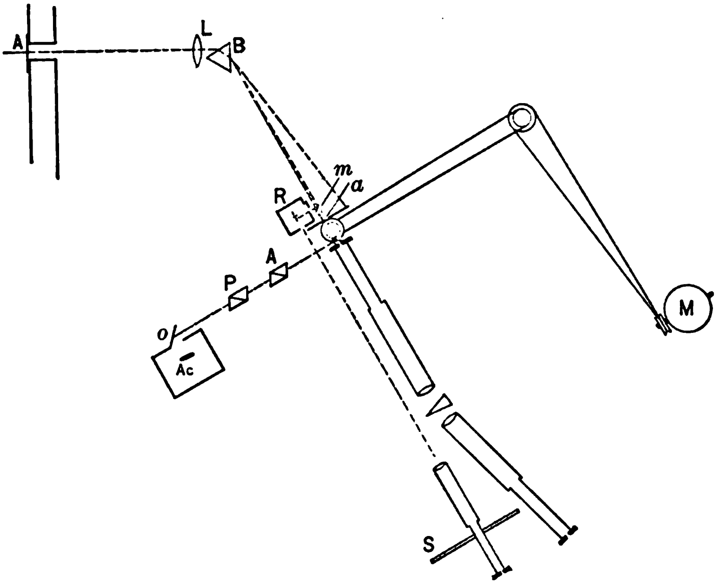

Fig. 1.

Finally, the spectra of the phosphorescent light and of the diffused light from the magnesium oxide surface were formed, one above the other in the field of the eye-piece of spectrometer, by a low dispersion prism of crown glass with a 30° angle. It was necessary to use such small dispersion as this prism gave because of the feebleness of phosphorescent light. The eye-piece contained the usual narrow aperture for cutting out of view all parts of the spectra except those which it was desired to compare. Then when the chosen parts of the spectra were made of the same intensity by adjusting one of the Nicol prisms, the intensity of phosphorescence at this wave-length was measured by $\sin^2\theta$, where θ is the angle between this position of the Nicols and the crossed position.

The cylinder *T* was rotated at a constant speed by a small motor *M* driven by current from storage cells. By the use of a system of pulleys the speed could be varied from about 0.03 sec. to 7 or 8 sec. per revolution.

As soon as readings were commenced it was found that the arc lamp as a source of exciting light would have to be discarded; its fluctuations were much too rapid to be followed by the luminosity of the sulphide. Observations could be taken only with sunlight on clear days.

On account of the late date at which the necessary apparatus was completed and, too, because of the scarcity of clear sunshine, I have been unable to make observations on more than one material—calcium sulphide.

2. Observations.

To afford a means of correcting for uncontrollable variations in the intensity of the exciting light it was necessary to establish, first of all, a relation between the intensity of phosphorescence and the intensity of illumination. The intensity of the exciting light was varied by interposing pieces of blue glass in the path of the ray of sunlight and was measured by the deflections of the radiometer, Mr.

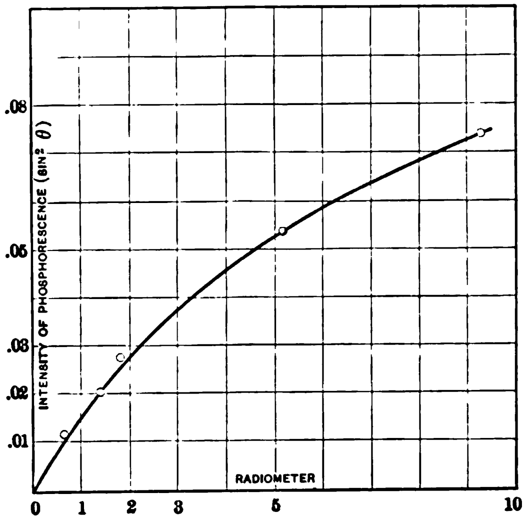

Fig 2.

Nichols having found that, under proper conditions, the deflections were proportional to the energy of radiations.

In obtaining these preliminary readings, the carbon bisulphide prism, which was used in order to obtain sufficient separation of the wave-lengths of sunlight within the necessarily short distance *BT*, gave some trouble. The action of the light in traversing the prism decomposed the bisulphide, setting free small particles of sulphur which diffused light in all directions. Some of this stray light, traveling in the direction of *m* and *a* caused an increase in the deflection of the radiometer but no corresponding increase in the luminosity of the phosphorescent material. A couple of curves obtained with bad charges of bisulphide are shown in Figs. 2 and 3.

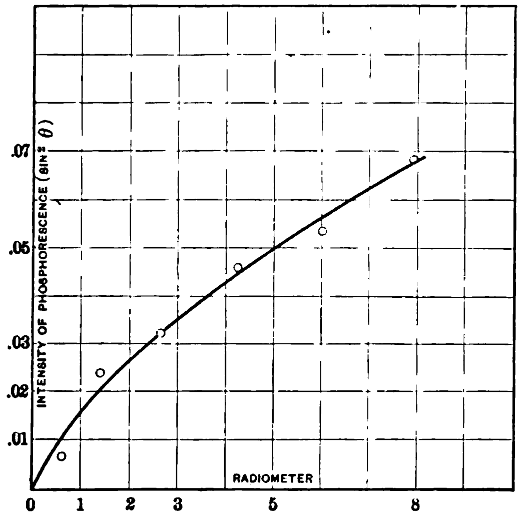

Fig 3.

Each point is obtained from averages of from three to five readings of the radiometer and of the Nicol prisms. To obtain the intensity of phosphorescence from the Nicol prism readings it is sufficiently accurate to take the square of the sine of the average angle.

A set of readings taken with a fresh charge of bisulphide is given in the table following; it shows to what degree of accuracy the settings of the Nicols were made. The errors in this column of readings made it desirable to have a larger number of observations in each group, but time did not permit.

Radiometer.			**Nicol Prisms.**		
Readings.	**Zero.**	**Deflection.**	**θ**	**Av. θ.**	**Sin^2 (Av. θ.)**
11.4 11.3 11.4	3.9	7.47	18.0 16.0 16.8	16.9	.0853
10.8 10.7 10.7 10.8	4.0	6.75	16.8 15.4 16.2 15.8	16.05	.0765
9.1 9.2 9.1	4.0	5.13	13.0 12.8 12.8	12.87	.0500
7.4 7.2 7.2	4.0	3.27	11.8 12.2 11.8	11.93	.0432
6.9 6.9 6.9	4.0	2.90	10.2 9.8 10.2	10.07	.0315
6.2 6.1 6.3 6.2	4.0	2.20	8.2 9.0 7.8 8.0	8.25	.0210
5.1 5.1 5.1	4.0	1.10	6.2 7.4 6.8	6.80	.0140

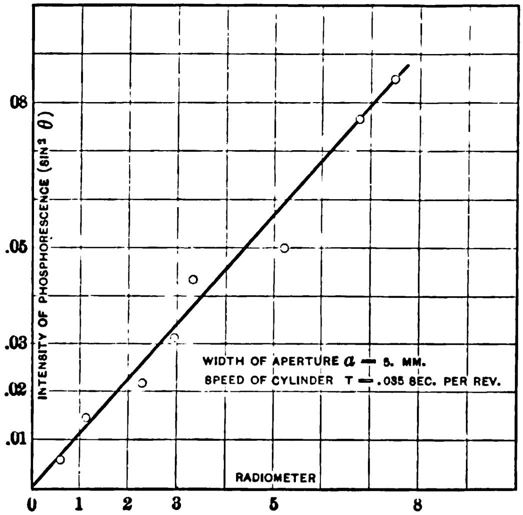

Fig 4.

The curve of Fig. 4 is plotted from this set of readings and indicates that the luminosity of the sulphide is proportional to the intensity of illumination, under the conditions of the present apparatus. Fig. 5 gives another curve of the same type, taken with a different width of aperture *a* and slightly different position of the spectrum of sun light. For all wave-lengths of exciting light, widths of aperture *a*, and speeds of rotation which were tried it was found that the curves of intensities were straight lines.

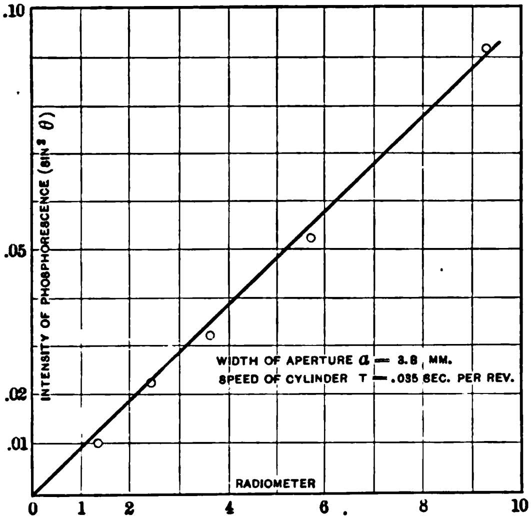

Fig 5.

Then keeping the intensity of illumination constant, or correcting, by means of the above curves, for the uncontrolable variations, and keeping the speed of rotation constant, the effect of varying the width of *a* was determined. During these observations the intensity of the exciting light would, of course, vary somewhat. The variation was corrected for by dividing $\sin^2\theta$ by the radiometer deflection, which operation is directed by the proportionality existing between the intensities. Fig. 6 is a curve showing the relation between the luminosity and width of aperture.

The relation between intensity of phosphorescence and speed of rotation of the cylinder is shown by Fig. 7. Here each point is obtained from an average of from eight to ten observations.

With the width of aperture *A*, which it was necessary to use, the spectrum of the sunlight was so impure that most of the wave-lengths, which were efficient in producing phosphorescence, were to be found in the aperture *a*. On account of this it was found impossible to obtain accurately by measurements the relation between the luminosity and the wave-length of exciting light. For this purpose a flat surface was painted with the sulphide of calcium and exposed in the spectrum from a narrow aperture. By noticing the luminosity soon after illumination a visibility curve was sketched showing approximately the desired relation.

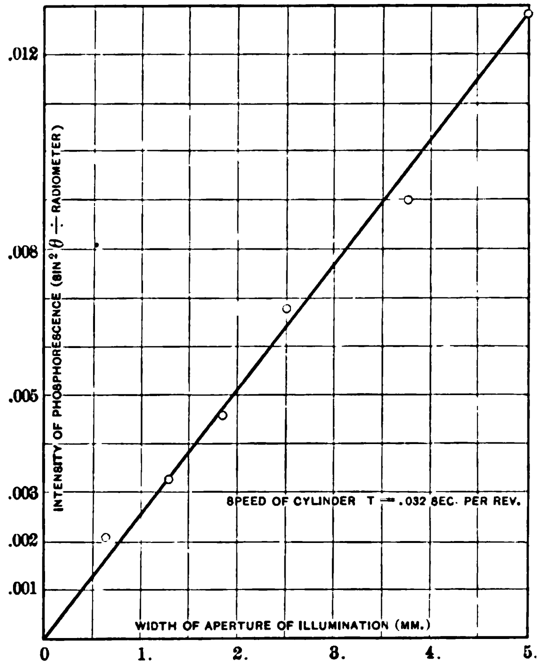

Fig 6.

And in the case of constant conditions of the cylinder *T*—as to illumination and speed—it was useless to compare the intensities at various regions of the spectra of the emitted light and of the light from the surface *o* because of the uneven distribution of energy in the spectrum of the acetylene flame. A visibility curve of intensity and wave-length of phosphorescent light was made.

These last two curves are drawn in the same figure (Fig. 8) in order to show more clearly the relation between the wave-lengths of exciting and emitted light. The ordinates of the lower curve represent the intensity of the whole group of wave-lengths of the

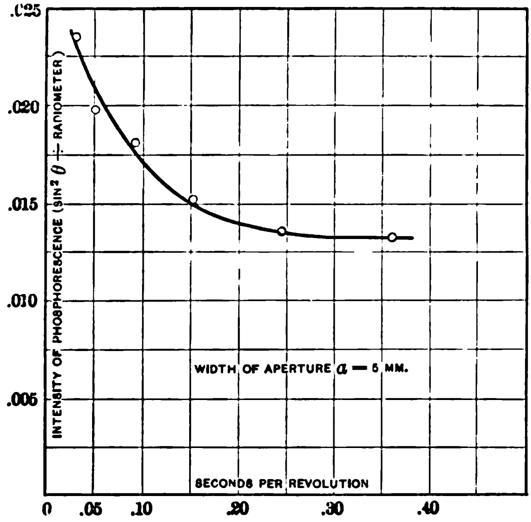

Fig 7.

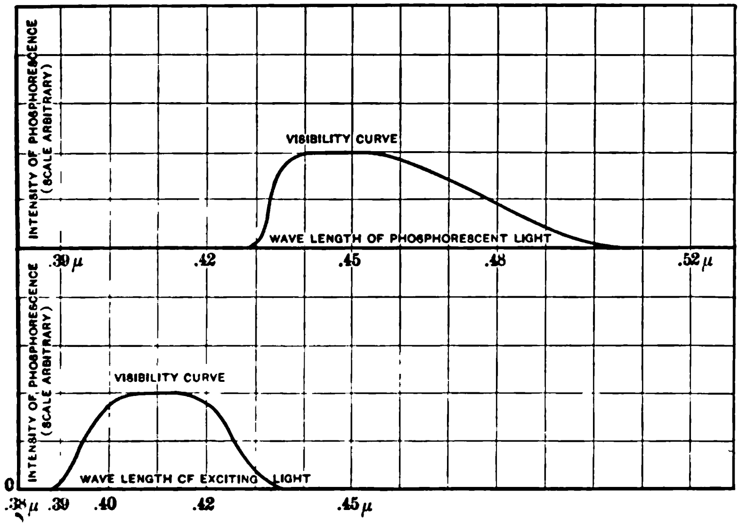

Fig. 8.

phosphorescent light. With a wider dispersion of the illuminating light, giving greater purity of spectrum than the present arrangement of apparatus affords, and thereby permitting of measurements on the wave-lengths of exciting light, it will be possible to separate this curve into its component curves and, by so doing, show quite definitely the relation between the wave-lengths of exciting and of emitted light. This relation would be of special interest in those sulphides which have more than one band in their spectra.

It may be shown by a discussion of the curve of intensity of phosphorescence and time of revolution of the cylinder, in connection with the curves of phosphorescence and width of aperture, that there are no definite curves of increase and decrease of luminosity during and after illumination; in other words, the rate of increase or of decrease at any intensity of phosphorescence depends upon the previous treatment which the luminous material has undergone. This is a fact which E. Wiedemann[1] had noticed in the case of Balmain's luminous paint and which he stated to be true of the sulphides of the alkaline earths, either from a knowledge of the constituents of the paint or from independent observations made on these sulphides.

The peculiarity may be shown by means of the present curves as follows: Assume that there are definite curves such as shown in Fig. 9, curve (1) being for increase and (2) for decrease of luminosity. Then when the cylinder of material has reached a steady

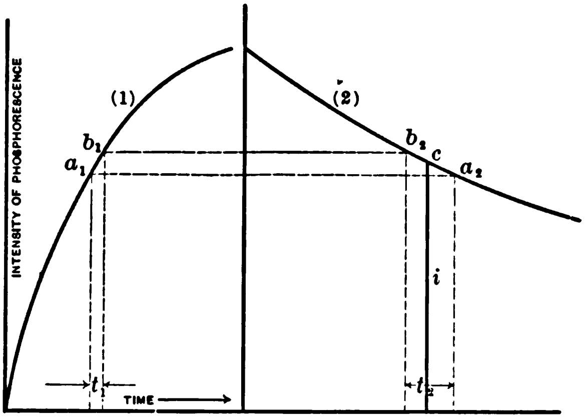

Fig. 9.

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[1] Weid. Annalen, 37, 177; 1889; Phil. Mag., 15-28, 149, 1889.

condition for a certain speed of rotation and a certain intensity of illumination, the material is going through some such cycle as a_1 b_1 b_2 a_2 for each revolution of the cylinder. Observation by the spectrometer is made as the material passes the slit, namely, at the condition represented on the curve (2) by the point c. Now since the increase in intensity from a_1 to b_1 is equal to the decrease from b_2 to a_2, the ratio of the slopes of the two curves at this intensity is equal to the inverse ratio of the times of illumination and insulation. But we can work at the same intensity of phosphorescence by decreasing the width of the aperture a and increasing the speed of rotation; in so doing we have changed the above ratio of times and consequently have no determined slope for the curves at the intensity considered.

So far, therefore, as the present observations are concerned the behavior of the calcium sulphide will have to be defined in terms of the apparatus used. There seems to be no way of so combining the results as to show the behavior in a more absolute manner. For this reason it is unnecessary to give the various times of illumination and of insulation in more detail than is done in the preceding pages and accompanying figures.

DEPT. of PHYSICS, CORNELL UNIV., June, 1899.

THE PHOTOMETRIC STUDY OF MIXTURES OF ACETYLENE AND HYDROGEN BURNED IN AIR.

BY LEON W. HARTMAN.

THE steady advance of acetylene as an illuminant justifies any investigation of the light and color properties of the acetylene flame burned under varying conditions. This investigation will be devoted primarily to studying the photometry and the spectrophotometry of the flame produced by burning in air varying mixtures of acetylene and hydrogen.

Very little reliable information concerning the cost and efficiency of acetylene as an illuminant has been given in recent years. Violle[1] found that an acetylene burner giving 100 candle power under a pressure of 30 cm. water, was twenty times more efficient than a coal gas burner of the Bengel type giving one carcel, or 9.6 candles per 105 liters; and at least six times more efficient than the same coal gas, burned in an Auer burner, giving one carcel per 30 liters of gas burned. Although in 1896 acetylene cost in Paris five times as much per cubic meter as ordinary illuminating gas, yet it was more economic as an illuminant since it gives fifteen times more light for an equal volume.[2] Hospitalier[3] made a comparison of the cost per carcel hour between a number of illuminants, from which he concludes that acetylene is about one-third as expensive as the incandescent lamp and about one-fourth as costly as ordinary gas burning in the butterfly tip. In this connection the results obtained by Wedding are interesting.[4] He studied the effect upon candle power of adding acetylene in varying proportions to ordinary illuminating gas. He found, for example, that without acetylene the burner consumed fifteen liters per bougie hour, and that with twelve parts of acetylene per hundred the burner burned four liters per bougie hour. His results are indicated in Fig. 1, where specific consumption, or the amount consumed per hour, is plotted as ordinates and the percentage of acetylene as abscissæ.

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[1] L'Éclairage Électrique, VI., p. 319, 1896.

[2] L'Éclairage Électrique, VI., pp. 133–4, 1896.

[3] Do.

[4] L'Éclairage Électrique, VI., p. 88, 1896.

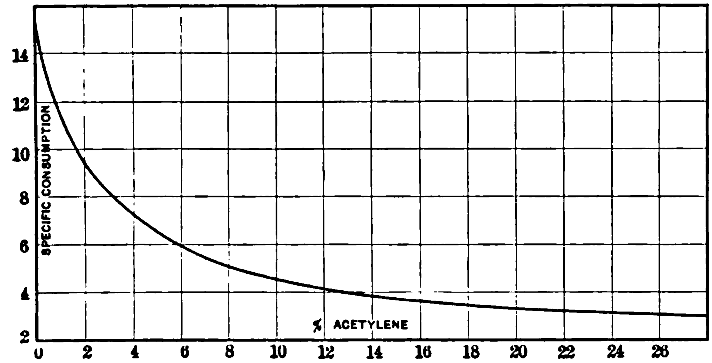

Fig. 1.

Wedding's curve, showing relation between specific consumption and percentage of acetylene in illuminating gas.

It has been found that the gas as it is given off from the acetylene generator contains impurities. In a number of the continental cities of Europe where this gas is used on a large scale as an illuminant, the removal of these impurities was a serious problem. It is well known that the peculiar pungent odor of common acetylene is due to the presence of phosphoretted hydrogen. To remove this, Lunge and Cedercreuze suggest the use of calcium hypochlorite.[1] More serious still are two other impurities found in ordinary acetylene, viz., arsine and hydrogen sulfide. In a series of analyses of common acetylene Giraud found[2] in a cubic meter of gas these impurities in sufficient quantities to warrant considerable prudence in the use of the gas.

Several methods of purifying the gas have been suggested. The following simple method of removing these impurities has been used by Berge and Reycher.[3] The gas from the generator was first passed through a solution of caustic potash. This removed the hydrogen sulfide by the precipitation of potassium sulfide. The gas was then passed through two Wolff bottles in series each containing a solution of mercuric chloride, which had been slightly acidified by adding dilute hydrochloric acid. Probably the resulting products were the phosphide and the arsenide of mercury, in which there was doubtless some crystalline mercuric chloride. After passing the gas through these solutions Giraud found it free from these obnoxious impurities. Their method was followed in this investigation whenever purified acetylene was used.

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[1] Electrician, XLI., p. 517.

[2] L'Éclairage Électrique, VIII., p. 319, 1896.

[3] Bulletin de la Société Chemique de Paris, XVIII., p. 218, 1897.

Turning to the photometry and the spectrophotometry of acetylene, one is confronted by the fact that there is an exceedingly small amount of published matter on this phase of the subject. Violle[1] mentions some photometric and spectrophotometric work which he did with acetylene. The results of his photometry have already been given.[2] As a result of his other work he says: "The spectrophotometer shows besides that in all the extent of the spectrum from *C* to *F* that the light of acetylene differs little from that of fused platinum." He describes the burner used and the diaphragm in front of the flame; this part of his apparatus was almost identically the same as the secondary standard used in this investigation. This standard will be described later. Fery, "On the photometry

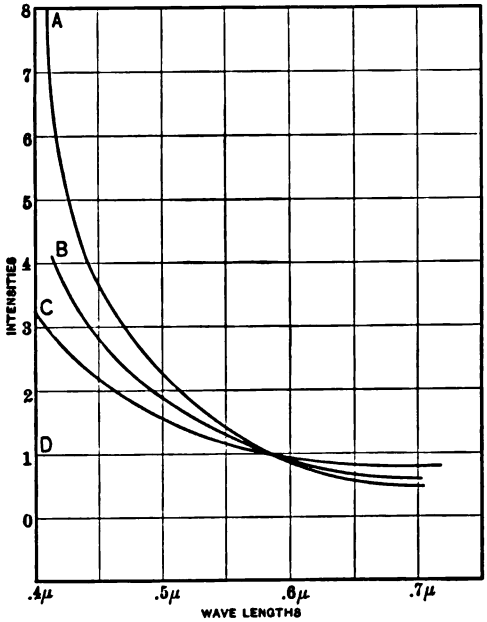

Fig. 2.

Spectrophotometric curves by Sharp. A—Acetylene in oxygen. B—Acetylene, 50%, and hydrogen, 50%, in oxygen. C—Acetylene in air. D—Kerosene lamp.

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[1] C. R., CXXII., p. 79–80; L'Éclairage Électrique, VI., p. 178, 1896.

[2] Vide, p. 1.

of acetylene,"[1] used the Carcel lamp as a standard; also the Drummond and magnesium lamps. He likewise gives neither data nor curves.

The Physical Department placed at my disposal a lantern slide on which were spectrophotometric curves[2] of acetylene burning in oxygen, acetylene fifty per cent. and hydrogen fifty per cent. burning in oxygen, acetylene burning in air, all of which sources were compared with the kerosene lamp as a standard. Inasmuch as these curves are very closely related to the subject under consideration, they have been incorporated in this résumé and are given in Fig. 2.

Apparatus and Methods.

The acetylene used in this investigation was generated in a generator designed by Dr. Sharp of the Physical Department and made expressly for the work in this laboratory. The gas was stored in tanks over water saturated with acetylene.

The mixtures of acetylene and hydrogen were likewise stored in tanks over water saturated with acetylene. At first thought, this might appear to be an unsatisfactory method since the percentage composition would be very apt to change, either by the water in the tank absorbing or giving off acetylene. Professor Lewes[3] found, in a similar mixture stored over water, that the proportion of acetylene present in the gas, as determined by analysis at the burner, was not the same as in the gas within the holder, although the water in both meter and holder was saturated as far as possible with the gas. Yet, as his analyses show, the precaution of analyses at the burner was an important one. Accordingly in each case as the gas came off from the tank, after having been thoroughly mixed, two or more analyses of the mixture were made at the burner. In analyses made at the beginning and at the end of a set of observations, in which time two or three hours might elapse, no noticeable change was observed. If an interval of several days intervened between two analyses of a given mixture, a change of two or three per cent. might occur.

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[1] L'Éclairage Électrique, VIII., p. 54, 1896.

[2] These curves were plotted by Dr. C. H. Sharp from data that he had taken in this laboratory.

[3] Chemical News, LXXI., p. 190, 1895.

The method of analysis was as follows: It was known that an ammoniacal solution of cuprous chloride would absorb acetylene, forming copper acetylide, while the hydrogen would not be affected. Hence measuring out 100 c.c. of the mixture in a Hempel burette and then passing the gas into a compound Hempel pipette for solids and liquids containing such an ammoniacal cuprous chloride solution, the acetylene would be wholly absorbed. Upon passing the remainder of the gas back into the burette the percentage of acetylene could be read directly. The preparation of the ammoniacal cuprous chloride solution was according to Sandmeyer's method.[1]

Throughout this work two general types of burner have been used. One form of burner was made by drilling a very small hole through a tightly fitting brass plug, which replaced the lava tip in an ordinary gas burner. The second type was the Nephey burner. Three forms of this were used; a single, a double and a triple burner. Figs. 3 and 4 will help to explain this form of burner. The essential feature is the lava tip, of which Fig. 3 is an enlarged lateral section and Fig. 4 is a trans-section. The outlet of the tip is a minute passage through the lava, ending in a chamber at the apex of a flat cone. Several passages, *a*, at regular intervals extend from this chamber to the atmosphere outside, thus allowing the gas to mix with air before it escapes through the larger bore, *b*, at the end of the tip. The single tip gives an elongated cylindrical flame ending in a tip very similar in shape to that of the Bunsen burner. In the triple burner, the three tips are arranged as in Fig. 5. Each flame impinges upon the others at an angle, thus producing a broad fan-shaped flame, whose plane is at right angles tothe plane containing the three tips. In the double burner the central tip of the triple burner is wanting. The purpose of this type of burner is, obviously, to secure complete combustion; it was therefore used when the percentage of acetylene was high.

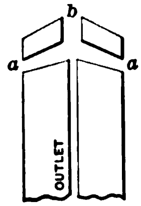

Fig. 3.
Lateral Section of Lava Tip.

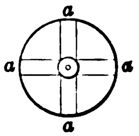

Fig. 4.
Trans-section of Lava Tip.

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[1] Berichte der Deutschen Chemischen Gesellschaft, XVII., p. 1633 b.

As a primary color standard in the spectrophotometric work, the Hefner lamp was used. As a primary standard in the photometric work, a standard incandescent lamp was used. As a secondary standard, however, the triple Nephey burner was found to be very satisfactory. It was observed that the central portion of the flame appeared uniform throughout a considerable range of pressure. Touse this limited portion of the flame, an half-inch diaphragm was fastened on the burner so that the diaphragm was in front of this uniform area. Thus, the triple Nephey burner under a given pressure was used as a secondary standard in the spectrophotometric work.

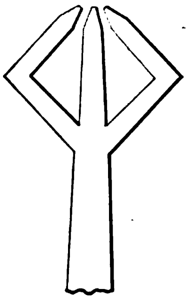
Fig. 5.
The Triple Burner.

To obtain the various measurements of gas pressure, a pair of burettes graduated in tenth centimeters and bent into U-tubes, were filled with colored water and used. By means of a glass Y in the tube leading to the burner, the pressure on the burner could be measured. To prevent rapid evaporation of the water the free end of the U-tube contained a cork having a small groove cut along its sides. Whenever measurements involving volume of gas were required, a standard Elster gas meter was used.

For the photometric work a track divided into one thousand divisions was used. Upon this track the photometer could be moved back and forth until equal intensities were secured, and the reading was then taken. The lamp at one end of the bar being a standard 16 candle-power incandescent lamp, the intensity of the unknown source could be readily computed. Two photometers were used: a Lummer-Brodhun and a Bunsen disk photometer. The former was preferable in cases where the color of the two sources was alike, and in cases where the intensities were weak. The disk photometer gave better service when the intensities were strong, and when the color of the two sources was unlike.

The stationary horizontal slit spectrophotometer used in this investigation was devised by Professor Nichols and is fully described elsewhere.[1]

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[1] PHYSICAL REVIEW, Vol. II., No. 8; Nichols' Laboratory Manual, Vol. II., p. 360.

The first results of photometry with the secondary standard were very unsatisfactory. The candle-power for given pressure varied more or less from day to day. To remedy this error a gas cushion was introduced in the tube leading from the Y to the burner. This gas cushion was made by soldering into the ends of a liter tin can two pieces of metal tubing, over which the rubber tubing of the burner fitted tightly. At the orifice of the can nearer the burner was a minute diaphragm. The result of this change was to produce a more constant flow of gas and to give a uniform candle-power under a given pressure. It is needless to say that at each observation the primary standard (*i. e.*, the standard 16 candle-power incandescent lamp) was adjusted at 16 candle-power by bringing its voltage to the proper value, and the unknown source was always carefully adjusted in position.

In the first series of spectrophotometric measurements, considerable difficulty was experienced in getting the two fields of the telescope exactly uniform in intensity. Although the field is very narrow and therefore free from a mixture of different wave-lengths, yet its great extent from right to left made it extremely difficult to decide when the two fields were equally bright. For example, if the eye could be held in a position where the line of sight to the narrow dark division between the two fields was coincident with the principal axis of the lens system of the telescope, there would be no difficulty; but if, when the intensities of the two fields were the same, the head were moved slightly to the right, the left field would appear the brighter; and *vice versa*. To overcome this a thick wooden cap was fitted over the eyepiece of the telescope and through the center of this cap a hole about ½ millimeter in diameter was bored. This cut down the length of the field within narrow limits. To see the line of division between the two fields after this change, the line of sight had to approximately coincide with the principal axis of the lens system of the telescope.

To avoid any instrumental error in the spectrophotometric work, the standard source and the unknown source were frequently interchanged. Accordingly, two sets of data were often obtained for a given mixture. The curves showed no marked change in form as a result.

CURVES AND RESULTS.

The calibration of apparatus was naturally the first work to be performed. The spectrophotometer was calibrated by identifying the principal Fraunhofer lines of the solar spectrum, whose wave-lengths were known, and noting the position of the telescope on the graduated scale along which it moves. Plotting wave-lengths for abscissæ and scale readings for ordinates a straight line resulted. The Vierordt slits of the spectrophotometer collimator were also calibrated by measuring under a microscope the width of the slits for a given turn of the screw.

It was found desirable to know the rate of flow of gas with change of pressure. To determine this, the single tipped Nephey burner was calibrated by allowing the gas to flow through a standard Elster gas meter in series with the burner. Thus, choosing certain pressures and keeping them constant for five or ten minutes, and then noting the exact volume of gas that passed in the given time, one had sufficient data for a curve. Plotting liters per minute as ordinates and pressures in centimeters as abscissæ, the resulting curve was approximately a parabola. Calibrating the half-foot Nephey burner likewise, another parabola was obtained. In these measurements the pressures were carried up until the flames began to hiss violently. As soon as hissing begins the parabolic form of curve does not appear to continue.

In this connection it will be interesting to note the results that were obtained by burning purified acetylene, under varying pressures, as a standard of light. The burner used was the triple Nephey burner with the diaphragm. As a result of a series of measurements, taken under varying atmospheric conditions, the curve in Fig. 6 was drawn. With but few exceptions the variation of the measurements from this curve was less than one per cent. These reults suggest the possibility of using purified acetylene, burning under the conditions described, as a standard source of candle-power. This phase of the subject is worthy of a more thorough investigation than time permitted here. The method of purifying the gas has already been described.

Reference has been made to the difficulty encountered in making the spectrophotometric measurements and to the method adopted in overcoming it. But still there was a possibility of a considerable range in making the settings of the slit. To lessen the probability of error in these measurements, the following criterion was adopted. The slits on the side of the unknown source were opened to a given position for the set of observations in question. Three settings of the other slits were then made, in which great care was exercised to get the two fields of the telescope exactly the same in intensity, and then the mean of these three readings was taken. In turn several settings of these slits (*i. e.*, the slits on the side of the secondary standard) were taken, until eight readings were obtained, no one of which varied from the mean of the first three readings by more than six or eight divisions. The mean of these eight readings was then taken as the width of the slits on the side of the secondary standard. The drum on the side of the unknown source was generally set at 50 or 100 divisions.

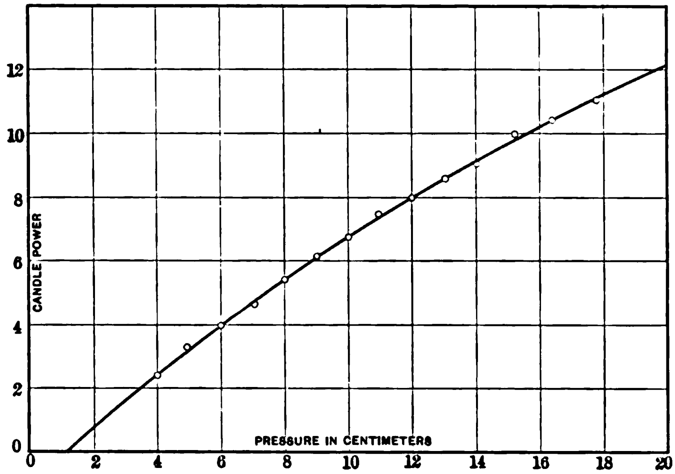

Fig. 6.

Candle-power. Pressure curve with purified acetylene.

It is obvious that if I_s denote the intensity of the secondary standard and I_x the intensity of the unknown source, and if W_s denote the width of slit set for the standard and W_x the width for the unknown source, the following relation is true

$$I_s : I_x :: W_x : W_s.$$

Therefore $$I_x = \frac{I_s W_s}{W_x}.$$

If we consider I_s unity, the intensity of the unknown source may be taken as the ratio of the slit widths. Plotting wave-lengths as abscissæ and intensities, or ratio of slit widths as ordinates, a set of curves was obtained. Finally, the value of the ordinate corresponding to the *D* line was taken from each curve and called unity. Each point upon the given curve is then reduced to the same scale by using the value of the ordinate corresponding to the wave-length 0.589 μ as a dividing factor. Plotting these new ratios as intensities and wave-lengths as abscissæ, another set of curves was obtained, a few of which are given in Fig. 7.

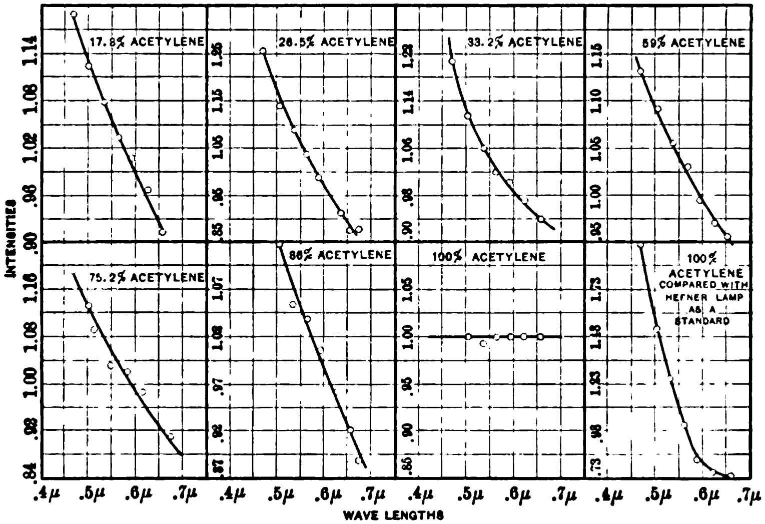

Fig. 7.

Hydrogen-acetylene spectrophotometris curves.

From a study of these curves it will be seen that the acetylene-hydrogen flame is richer in the short wave-lengths than the flame of the secondary standard burning common acetylene. Moreover, the color properties of the flame appear to be independent of the amount of hydrogen in the mixture. Upon going to the limit this statement would not hold true.

It may be well to note that in these curves, by percentage of acetylene is meant the percentage of acetylene by volume as determined by two or three analyses of the given mixture. These analyses were usually made within an hour of each other and the mean was then taken. If the percentage of acetylene in the mixture was above forty per cent., the lava tip was generally used at a gas pressure of ten centimeters; if the percentage of acetylene was below forty per cent., the brass tip was used at a gas pressure of four centi-

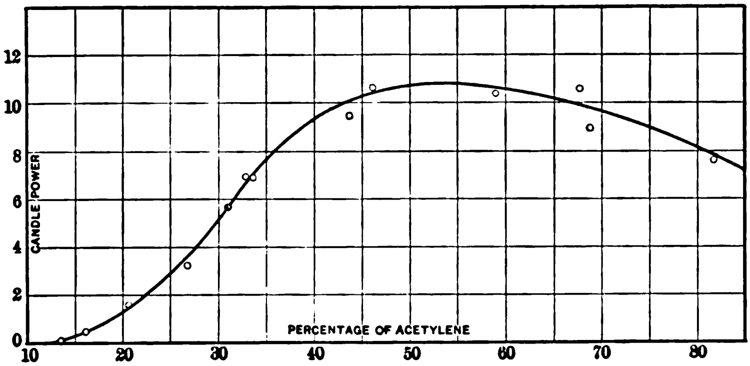

Fig. 8.

Variation of candle-power of the brass tip with change of percentage of acetylene in the mixture.

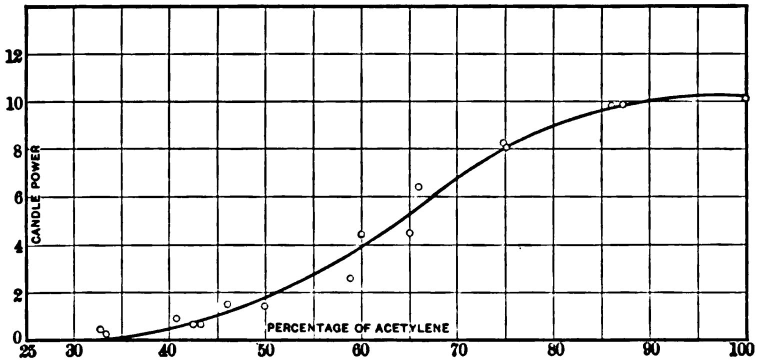

Fig. 9.

Variation of candle-power of the small lava tip with change of percentage of acetylene in the mixture.

meters. The brass tip was used, however, to obtain candle power throughout a wide range of percentages.

If the percentage of acetylene in the various mixtures be plotted as abscissæ and the candle-power of the brass tip be taken as ordinates of one curve and the candle-power of the lava tip be taken as ordinates of another curve, the curves in Fig. 8 and Fig. 9 are obtained. In the case of the brass tip, it will be seen that the candle-power reaches a maximum and then falls away with increasing percentage of acetylene. This is due to the incomplete combustion of the gas after a given percentage of acetylene in the mixture has been reached. In case of the lava tip, the flame, with low percentages of acetylene, appears much like the flame of burning hydrogen; at first, it slowly increases in candle power with increasing percentage of acetylene, and does not reach the stage of incomplete combustion as is shown by the curve.

In conclusion, I desire to call attention to the difficulty that was experienced, in the first series of observations, in determining the curvature of the curves obtained from the spectrophotometric measurements. For example, one would hesitate before saying that the two curves in Fig. 10 are incorrect, yet their curvature is directly

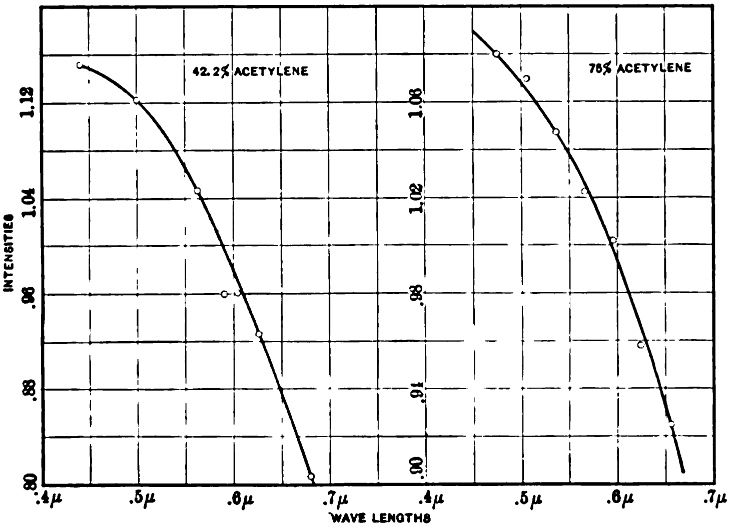

Fig. 10.

opposite to that in the curves of Fig. 7. The results of my first measurements, giving the uniform curve on the left side of Fig. 10, prejudiced me for a time in favor of that form of curve. The majority of my curves, however, had a downward inflection. This led me to plot all of the curves of doubtful curvature with as much of a downward inflection as I could consistently give them. I am inclined to believe, however, that the amount of curvature has but little significance. These two curves are the only ones that have been rejected because of their curvature and they are introduced here to show what apparently consistent curves it is possible to get in similar measurements.

Physical Laboratory of Cornell University, June 30, 1899.

A PHOTOGRAPHIC STUDY OF ELECTROLYTIC CELLS.

By Rolla R. Ramsey.

WHILE making preliminary experiments with a view of using Töpler's "Schlierenapparat"[1] as a possible means for the study of vibrating films as suggested by Dr. A. G. Webster, the idea occurred to me of using the apparatus to study electrolytic action. The apparatus, as shown in diagram, is arranged as follows: S_1 is an illuminated adjustable slit, the image of which, by means of the lens, L_1, is thrown on a second adjustable slit, S_2. An electrolytic cell, C, is placed between L_1 and S_2 near to L_1. The image of C is projected on the plate in the camera at P by means of two lenses L_2 and M. L_1 is an achromatic lens of 5 cm. diameter and 93 cm. focal length. L_2 is a photographic lens of 25.5 cm. focal length. M is a 3 cm. focal length microscope objective. S_1 is illuminated with sunlight by means of a cylindrical lens L_3 and a heliostat H.

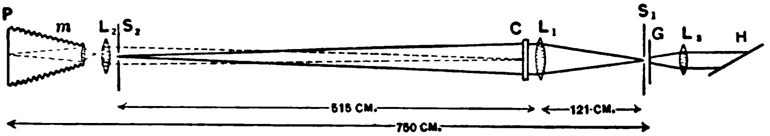

Fig. 1.

The principle of the "Schlieren-methode" is, that if the medium between S_1 and S_2 is homogeneous, then the image of S_1 will be brought to a sharp focus at S_2, which has been placed at the conjugate focus of S_1. If the slit of S_2 is made to coincide with the image of S_1, then the illumination on the screen P will be uniform. Moving the slit S_2 up will intercept the light and the screen will suddenly become dark. If the medium between S_1 and S_2 is not homogeneous, then the image of S_1 will not be sharp and moving S_2 up will intercept the light from certain portions of the field, causing it to be blotched or spotted, "Schlieren." Intercepting the lower part of the image will cause certain portions to be light, while intercepting the upper will illuminate what was dark in the first case and darken the former bright portions. The homogeneity of medium will be violated by heating the air, by inserting a cell in which liquids of different densities are placed, or by an electrolytic cell having a current flowing through it. The cell *C* is made of two pieces of plane optical glass, the inside dimensions of which are, width 2.2 cm., thickness .4 cm. The electrodes are made of strips of commercial sheet zinc. The electrolyte is a 10% solution of sinc sulphate (10 gm. $ZnSO_4$ in 90 cu. cm. of water). The solution is a clear liquid, which, when placed in the cell, has every appearance of water to the naked eye.

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[1] Pogg. Ann., 131, 33 and 180, 1867.

The accompanying photographs will illustrate the results. No. 2 shows the current passing up through the electrolyte. Since the anode is being dissolved and the cathode plated, we have changes of density at the two electrodes. A dense solution, which is seen to be falling, is formed at the anode. A corresponding light solution, which rises to the surface, is formed at the cathode. The portion between the two electrodes is uniform and, therefore, the appearance is dark. The light in this case is taken from the upper part of the slit. In No. 4 we have the same cell except that the current is reversed, that is, passing down through the solution. The dense solution is falling from the anode and the rare is rising, making a variable density between the two electrodes. Light is taken from the upper part of slit the same as in No. 2. No. 15 shows vertical electrodes. The current causes a stream of dense solution to flow from the anode. No. 15 also shows a characteristic stratification of the solution between the two electrodes. A horizontal layer of dense liquid is invariably formed at the lower extremities of the electrodes. Light is taken from the center of the slit. No. 11 is taken from a 5 cm. cubical cell made of plate glass with horizontal plate electrodes. The current is passing down through the liquid. The light is taken from the upper part of the slit. Nos. 32 and 33 illustrate the difference in the effect by taking light from the upper or lower portions of the slit. No. 32 was taken from the lower part of the slit, while No. 33 was taken from the upper. They were taken one after the other as quickly as possible, the only change being that of moving S_2 up a short distance. It will be noticed that one is the exact negative of the other.

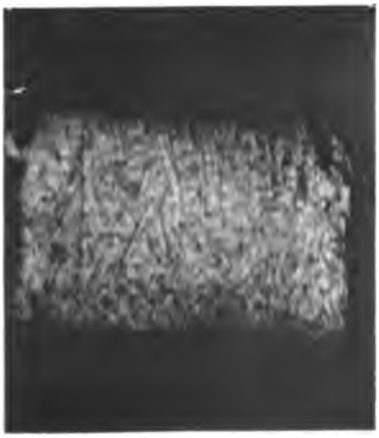
The time of exposure varied from one to four seconds, although plates were taken with an ordinary instantaneous shutter.

CLARK UNIVERSITY, JUNE 14, 1899.

No. 2

No. 4

No. 15



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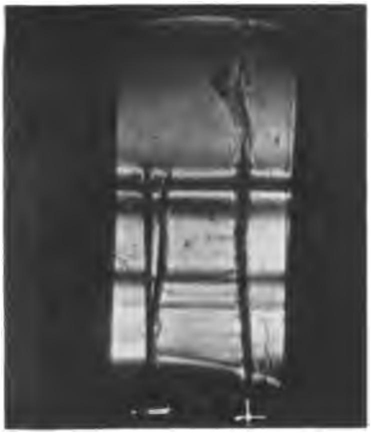

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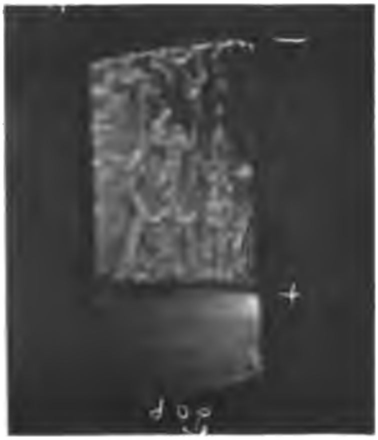

No. 33

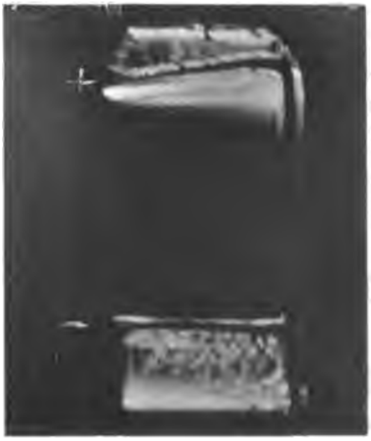

PLATE I. RAMSEY.

NEW BOOKS.

An Introduction to the Mathematical Theory of Attraction. FRANCIS A. TARLETON. Pp. xi+290. New York, Longmans, Green & Co., 1899.

The mathematical treatment of the theory of attraction from the point of view from which it has been actually developed, that of direct action at a distance, is necessarily of great intrinsic interest to the mathematician; but its prime importance to the mathematical physicist is due to the fact that the mathematical analysis and the actual body of formulæ which owed their origin to the old theory have proved to be applicable not only to electrostatics, but, with very little modification, to such apparent dissimilar subjects as the flow of fluids (including heat and electricity) and stress and strain in elastic solids.

Now it is true that electrostatics can be treated in part from the point of view of action at a distance, but many of the most important phenomena depend upon the condition of the intervening medium, the so-called "field"; and in fluid flow and elasticity what corresponds mathematically to the "field" is the direct object of study.

The Newtonian law of gravitation is so hammered into the schoolboy from his earliest days that when he comes to the systematic study of the theory of attraction the idea of action at a distance seems to him entirely natural and simple in spite of its philosophical difficulties, and on this account our author in his laudable endeavor to smooth the path of the student is undoubtedly right in approaching his subject from this side; but after the ground has once been broken too much emphasis cannot be laid on the study of the condition of the intervening medium, that is, of the field of force due to the presence of the attracting or repelling masses. It seems to me that the main respects in which the book under review is open to adverse criticism are the lack of this emphasis, the scarcity of problems where numerical results are demanded, and the absence of any allusion to vectors and vector-functions.

As it must always seem ungracious to find fault with an author who has given us much, because he has not seen fit to give us more, I hasten to speak of some of the many merits of the book. The definitions are clear and precise and not too numerous; the proofs are unusually direct and simple; the propositions are very well arranged, and, within the range to which the author has limited himself, include about all that even the advanced student is likely to need; and the problems are abundant even for an English treatise, and are well selected and instructive.

The consistent treatment of the "columnar" cases, as if there were a uniplanar distribution of mass acting inversely as the distance, is a prominent feature of the treatise, and one for which much can be said.

The book is certainly one from which the student who is thrown on his own resources can obtain a competent knowledge of the theory of attraction without waste of labor or loss of interest, and this is no small praise, especially, as in the words of the preface "It is a book for students not for professors." The skillful professor for whom it is not intended could perhaps use with greater effect on his class a judicious combination of Routh's Statics, Minchin's Statics, and Webster's Electricity and Magnetism. W. E. BYERLY.

CAMBRIDGE, July 10, 1899.

Stars and Telescopes. A Handbook of Popular Astronomy. By DAVID B. TODD. Pp. xvi + 419. Boston, Little, Brown & Co. 1899.

In this volume Professor Todd has gathered together the most important data of the new astronomy in a form intelligible to every reader. The allotment of space to astrophysics is relatively much larger than in previous treatises on astronomy, and this is a feature which will render the volume useful to the student of physics as well as to the student of astronomy. The book is copiously illustrated. There are numerous reproductions of modern star photographs and spectra, together with pictures of the various types of apparatus used in astrophysical work. There is a great range of merit in these illustrations, some of which are extremely satisfactory while others show all the worst features of half-tone reproductions directly from the photograph. The list of references to the original sources with which each chapter closes is an admirable feature.

E. L. N.

Volume IX. October, 1899. Number 4.

THE

PHYSICAL REVIEW.

ON THE MAGNETIC SHIELDING EFFECT OF TRI-LAMELLAR SPHERICAL AND CYLINDRICAL SHELLS.

BY A. P. WILLS.

THE subject of magnetic shielding has already received considerable attention, and, at the present day with the spread of the trolley system and the consequent increase of disturbing earth currents, the subject often demands attention from those having to do with suspended magnetic systems of any sort.

For a long time it was the custom to use extremely thick single iron shells as a protecting device. A single shell is in a measure effective, but in most cases one-tenth of the shielding material used could, if properly distributed, be made to produce many times the shielding actually produced by the usual distribution.

The advantage to be gained by using concentric shells separated by unmagnetic inter-spaces has been shown by Professor Rücker and by Professor du Bois. The former has given a general solution of the problem of multi-lamellar shielding with spherical shells; the results of this mathematical paper[1] are somewhat difficult to apply in actual shielding practice partly because there is involved the necessity of constant interpretation of the not very explicit equations used in the spherical harmonic analysis. Furthermore the practical advantages of cylindrical shells are much greater than those of spherical shells.

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[1] Rücker, Phil. Mag. (5), 37, p. 95, 1894.

(Convenient cylindrical shells may be made by rolling up thin sheet iron strips of proper width.) Professor du Bois[1] has treated the case of bi-lamellar shielding both for spherical and cylindrical shells, and has, moreover, applied the results of his theoretical and experimental investigations in the construction of multi-lamellar ironclad galvanometers.[2] Experience showed that in cases of considerable disturbance it was desirable to use three shields instead of two as a protecting device.

Acting upon the suggestion of Professor du Bois the present writer has deduced the general explicit formulæ giving the shielding effect of tri-lamellar spherical and cylindrical shells under conditions to be mentioned later and in such mathematical form as to be readily interpreted. These general formulæ have then been applied in the graphical discussion of a particular problem well adapted to show the great advantage to be gained by introducing suitable air gaps within the shielding material.

In virtue of the formal analogy in the treatment of the spherical and cylindrical problems the corresponding equations for the two cases are given together at each step in the development.

We shall suppose a uniform magnetic field of strength H_e to be impressed upon the system of shells. We shall deal with the case where the space *within* the shells is to be shielded against H_e. Further we shall suppose the axes of the cylinders to be perpendicular to the lines of force; that the spherical and cylindrical shells are concentrically arranged; that the permeability is constant and equal for all shells. This last assumption involves, of course, a restriction in the values of H_e to those not exceeding, say .01 C.G.S. units, producing small variations in the magnetic condition of the shells superimposed upon their intrinsic magnetization which may be zero or have a finite residual value. It is true that this limitation invalidates the discussion for strong fields; but when we remember that the trouble with galvanometers arises from disturbing fields of the order of magnitude mentioned we may rest assured that our formulæ are capable of practical application in the designing of shielding apparatus.

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[1] du Bois, Wied. Ann., 63, p. 348, 1897, and 65, p. 1, 1898. The Electrician, Vol. 40, 1898.

[2] du Bois and Rubens, Verhandl. physik. Gesellsch. Berlin, 17, p. 100, 1898.

Now, suppose a uniform disturbing field H_e to be impressed upon our tri-lamellar shielding systems, Fig. 1 representing both the spherical and cylindrical systems. The letters r_1, R_1, r_2, R_2, r_3, R_3, represent the inner and outer radii of the various shells; the small letters always referring to the inner radii and the large to the outer. We shall find a notation similar to that introduced by Professor du Bois in his treatment of the bi-lamellar problem to be convenient. It is given below in tabular form:

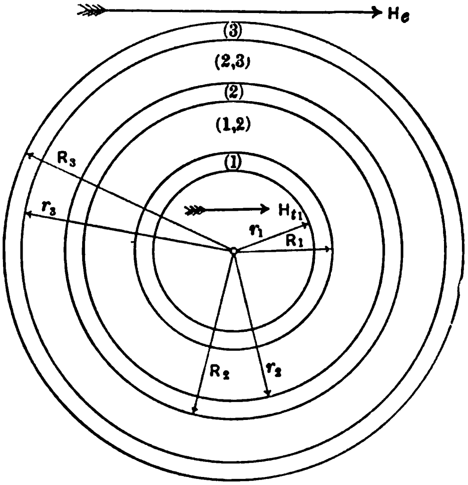

Fig. 1.

Spheres.		Cylinders.	
$p_1 = \frac{r_1^3}{R_1^3}$	$m_1 = 1 - \frac{r_1^3}{R_1^3}$	$q_1 = \frac{r_1^2}{R_1^2}$	$n_1 = 1 - \frac{r_1^2}{R_1^2}$
$p_2 = \frac{r_2^3}{R_2^3}$	$m_2 = 1 - \frac{r_2^3}{R_2^3}$	$q_2 = \frac{r_2^2}{R_2^2}$	$n_2 = 1 - \frac{r_2^2}{R_2^2}$
$p_3 = \frac{r_3^3}{R_3^3}$	$m_3 = 1 - \frac{r_3^3}{R_3^3}$	$q_3 = \frac{r_3^2}{R_3^2}$	$n_3 = 1 - \frac{r_3^2}{R_3^2}$
$p_{12} = \frac{R_1^3}{r_2^3}$	$m_{12} = 1 - \frac{R_1^3}{r_2^3}$	$q_{12} = \frac{R_1^2}{r_2^2}$	$n_{12} = 1 - \frac{R_1^2}{r_2^2}$
$p_{23} = \frac{R_2^3}{r_3^3}$	$m_{23} = 1 - \frac{R_2^3}{r_3^3}$	$q_{23} = \frac{R_2^2}{r_3^2}$	$n_{23} = 1 - \frac{R_2^2}{r_3^2}$

We shall consider the cylinders as infinitely long; but experience has shown that the equations to be derived hold in the central portion of cylinders whose length is not less than three or four times the radius.

Now suppose the total resultant field in the direction of H_e within the innermost shell to be denoted by H_{i1}. The ratio of H_e to H_{i1} may be taken as a measure of the effectiveness of the shielding device. Denote this ratio by g then $g = \frac{H_e}{H_{i1}}$. $g = \infty$ would, of course, mean perfect shielding.

Now it becomes necessary to find an equation expressing the relation existing among the various quantities entering into the problem, namely, "the shielding ratio" g, the permeability μ and the geometrical data given by the p's and m's in the case of the spherical shells, the q's and n's in the case of the cylindrical shells.

FUNDAMENTAL EQUATIONS.

Since Fig. 1 represents a plan section of our double system of shells, the axis of the cylindrical system will be perpendicular to the plane of the paper. The direction of the impressed field is indicated by the arrows. There will be produced upon each of the six surfaces of separation between iron and air surtace distributions of "free magnetism." In the theory of such distributions it is shown that a distribution upon a spherical or cylindrical surface produces within the surface, when the impressed force is uniform, a uniform magnetic field which is coincident with the impressed field but acting in the opposite direction and of different intensity. If S denote any one of the surfaces of the spherical system and H_s the field produced within S by the distribution upon S, the theory shows that at a point without S, P say, the radial force produced by the distribution upon S is $-2\frac{r^3}{R^3}H_s \cos \vartheta$, where r is the radius of S and R the distance from the center of S to the point P and the tangential force at P (perpendicular to the radial force) is $-\frac{r^3}{R^3}H_s \sin \vartheta$, where ϑ is the angle between the radius to P and the impressed field. With cylindrical shells, for any surface S and a point P without S we should have the corresponding forces $-\frac{r^2}{R^2}H_s \cos \vartheta$, and

$-\frac{r^2}{R^2} H_e \sin \vartheta$. Now considering the surface S we may think of the resultant force within it to be made up of two parts one of which is due to causes external and the other to causes internal. Call the first H_t and the second H_i. Starting in each case with the inner surface we may write down the following equations, applying to both spherical and cylindrical systems.

$$R \left\{ \begin{array}{l} H_{t1} = H_{t2} + H_{i1} = H_e + H_{i6} + H_{i5} + H_{i4} + H_{i3} + H_{i2} + H_{i1} \\ H_{t2} = H_{t3} + H_{i2} = H_e + H_{i6} + H_{i5} + H_{i4} + H_{i3} + H_{i2} \\ H_{t3} = H_{t4} + H_{i3} = H_e + H_{i6} + H_{i5} + H_{i4} + H_{i3} \\ H_{t4} = H_{t5} + H_{i4} = H_e + H_{i6} + H_{i5} + H_{i4} \\ H_{t5} = H_{t6} + H_{i5} = H_e + H_{i6} + H_{i5} \\ H_{t6} = H_{t7} + H_{i6} = H_e + H_{i6} \end{array} \right.$$

From what has been said above and from Fig 1 the meaning of the notation is clear.

We do not require to know the exact distribution of the lines of force throughout the shells but the quantities H_{i6}, H_{i5}, H_{i4}, H_{i3}, H_{i2}, H_{i1} must be such as to satisfy the two surface conditions namely: that at surfaces of separation of iron from air the tangential components of the magnetic force must be equal in the two media and that the normal component of the magnetic induction must be continuous. The first condition, from what has been already said, may be seen to be always satisfied whatever the values of $H_{i6} \cdots H_{i1}$. But in order that the condition of continuity in the normal component of the magnetic induction be satisfied we must have the following equations also satisfied. In these equations μ represents the permeability of the iron. The equations pertaining to the spherical shells are given first. In both sets of equations $\cos \theta$ occurs as a common factor in both members of each equation but is not written down.

$$
S \left\{
\begin{aligned}
&H_{t1} = \mu(H_{t2} - 2\,H_{i1}) \\
&\mu\left(H_{t2} - 2\,\frac{r_1^3}{R_1^3}\,H_{i1}\right) = H_{t3} - 2H_{i2} - 2\,\frac{r_1^3}{R_1^3}\,H_{i1} \\
&H_{t3} - 2\,\frac{R_1^3}{r_2^3}\,H_{i2} - 2\,\frac{r_1^3}{r_2^3}\,H_{i1} = \mu\left(H_{t4} - 2H_{i3} - 2\,\frac{R_1^3}{r_2^3}\,H_{i2} - 2\,\frac{r_1^3}{r_2^3}\,H_{i1}\right) \\
&\mu\left(H_{t4} - 2\,\frac{r_2^3}{R_2^3}\,H_{i3} - 2\,\frac{R_1^3}{R_2^3}\,H_{i2} - 2\,\frac{r_1^3}{R_2^3}\,H_{i1}\right) = H_{t5} - 2H_{i4} - 2\,\frac{r_2^3}{R_2^3}\,H_{i3} - 2\,\frac{R_1^3}{R_2^3}\,H_{i2} - 2\,\frac{r_1^3}{R_2^3}\,H_{i1} \\
&H_{t5} - 2\,\frac{R_2^3}{r_3^3}\,H_{i4} - 2\,\frac{r_2^3}{r_3^3}\,H_{i3} - 2\,\frac{R_1^3}{r_3^3}\,H_{i2} - 2\,\frac{r_1^3}{r_3^3}\,H_{i1} = \mu\left(H_{t6} - 2H_{i5} - 2\,\frac{R_2^3}{r_3^3}\,H_{i4} - 2\,\frac{r_2^3}{r_3^3}\,H_{i3} - 2\,\frac{R_1^3}{r_3^3}\,H_{i2} - 2\,\frac{r_1^3}{r_3^3}\,H_{i1}\right) \\
&\mu\left(H_{t6} - 2\,\frac{r_3^3}{R_3^3}\,H_{i5} - 2\,\frac{R_2^3}{R_3^3}\,H_{i4} - 2\,\frac{r_2^3}{R_3^3}\,H_{i3} - 2\,\frac{R_1^3}{R_3^3}\,H_{i2} - 2\,\frac{r_1^3}{R_3^3}\,H_{i1}\right) \\
&= H_e - 2H_{i6} - 2\,\frac{r_3^3}{R_3^3}\,H_{i5} - 2\,\frac{R_2^3}{R_3^3}\,H_{i4} - 2\,\frac{r_2^3}{R_3^3}\,H_{i3} - 2\,\frac{R_1^3}{R_3^3}\,H_{i2} - 2\,\frac{r_1^3}{R_3^3}\,H_{i1}
\end{aligned}
\right.
$$

The corresponding set of equations for the cylinders is

$$T\left\{\begin{aligned}
&H_{t1}=\mu(H_{t2}-H_{i1})\\
&\mu\left(H_{t2}-\frac{r_1^2}{R_1^2}H_{i1}\right)=H_{t3}-H_{i2}-\frac{r_1^2}{R_1^2}H_{i1}\\
&H_{t3}-\frac{R_1^2}{r_2^2}H_{i2}-\frac{r_1^2}{r_2^2}H_{i1}=\mu\left(H_{t4}-H_{i3}-\frac{R_1^2}{r_2^2}H_{i2}-\frac{r_1^2}{r_2^2}H_{i1}\right)\\
&\mu\left(H_{t4}-\frac{r_2^2}{R_2^2}H_{i3}-\frac{R_1^2}{R_2^2}H_{i2}-\frac{r_1^2}{R_2^2}H_{i1}\right)=H_{t5}-H_{i4}-\frac{r_2^2}{R_2^2}H_{i3}-\frac{R_1^2}{R_2^2}H_{i2}-\frac{r_1^2}{R_2^2}H_{i1}\\
&H_{t5}-\frac{R_2^2}{r_3^2}H_{i4}-\frac{r_2^2}{r_3^2}H_{i3}-\frac{R_1^2}{r_3^2}H_{i2}-\frac{r_1^2}{r_3^2}H_{i1}=\mu\left(H_{t6}-H_{i5}-\frac{R_2^2}{r_3^2}H_{i4}-\frac{r_2^2}{r_3^2}H_{i3}-\frac{R_1^2}{r_3^2}H_{i2}-\frac{r_1^2}{r_3^2}H_{i1}\right)\\
&\mu\left(H_{t6}-\frac{r_3^2}{R_3^2}H_{i5}-\frac{R_2^2}{R_3^2}H_{i4}-\frac{r_2^2}{R_3^2}H_{i3}-\frac{R_1^2}{R_3^2}H_{i2}-\frac{r_1^2}{R_3^2}H_{i1}\right)\\
&=H_{e}-H_{i6}-\frac{r_3^2}{R_3^2}H_{i5}-\frac{R_2^2}{R_3^2}H_{i4}-\frac{r_2}{R_3^2}H_{i3}-\frac{R_1}{R_3^2}H_{i2}-\frac{r_1^2}{R_3^2}H_{i1}
\end{aligned}\right.$$

TREATMENT OF FUNDAMENTAL EQUATIONS.

From the set of equations (S) we may eliminate the H'_i's with the aid of the set (R) giving for the spherical shells after combination and rearrangement of terms the following set:

$$(\mu-1)H_{i6}+(\mu-1)H_{i5}+(\mu-1)H_{i4}+(\mu-1)H_{i3}+(\mu-1)H_{i2}$$
$$-(2\mu+1)H_{i1}=-(\mu-1)H_e$$

$$(\mu-1)H_{i6}+(\mu-1)H_{i5}+(\mu-1)H_{i4}+(\mu-1)H_{i3}+(\mu+2)H_{i2}$$
$$-2\frac{r_1^3}{R_1^3}(\mu-1)H_{i1}=-(\mu-1)H_e$$

$$(\mu-1)H_{i6}+(\mu-1)H_{i5}+(\mu-1)H_{i4}-(2\mu+1)H_{i3}-\frac{2R_1^3}{r_2^3}(\mu-1)H_{i2}$$
$$-2\frac{r_1^3}{r_2^3}(\mu-1)H_{i1}=-(\mu-1)H_e$$

$$(\mu-1)H_{i6}+(\mu-1)H_{i5}+(\mu+2)H_{i4}-2\frac{r_2^3}{R_2^3}(\mu-1)H_{i3}-2\frac{R_1^3}{R_2^3}(\mu-1)H_{i2}$$
$$-2\frac{r_1^3}{R_2^3}(\mu-1)H_{i1}=-(\mu-1)H_e$$

$$(\mu-1)H_{i6}-(2\mu+1)H_{i5}-2\frac{R_2^3}{r_3^3}(\mu-1)H_{i4}-2\frac{r_2^3}{r_3^3}(\mu-1)H_{i3}-2\frac{R_1^3}{r_3^3}(\mu-1)H_{i2}$$
$$-2\frac{r_1^3}{r_3^3}(\mu-1)H_{i1}=-(\mu-1)H_e$$

$$(\mu+2)H_{i6}-2\frac{r_3^3}{R_3^3}(\mu-1)H_{i5}-2\frac{R_2^3}{R_3^3}(\mu-1)H_{i4}-2\frac{r_2^3}{R_3^3}(\mu-1)H_{i3}-2\frac{R_1^3}{R_3^3}(\mu-1)H_{i2}$$
$$-2\frac{r_1^3}{R_3^3}(\mu-1)H_{i1}=-(\mu-1)H_e$$

The corresponding set for the cylinders is

$$
\begin{aligned}
&(\mu-1)H_{i6}+(\mu-1)H_{i5}+(\mu-1)H_{i4}+(\mu-1)H_{i3}+(\mu-1)H_{i2}-(\mu+1)H_{i1}=-(\mu-1)H_e\\
&(\mu-1)H_{i6}+(\mu-1)H_{i5}+(\mu-1)H_{i4}+(\mu-1)H_{i3}+(\mu+1)H_{i2}-\frac{r_1^2}{R_1^2}(\mu-1)H_{i1}=-(\mu-1)H_e\\
&(\mu-1)H_{i6}+(\mu-1)H_{i5}+(\mu-1)H_{i4}-(\mu+1)H_{i3}-\frac{R_1^2}{r_2^2}(\mu-1)H_{i2}-\frac{r_1^2}{r_2^2}(\mu-1)H_{i1}=-(\mu-1)H_e\\
&(\mu-1)H_{i6}+(\mu-1)H_{i5}+(\mu+1)H_{i4}-\frac{r_2^2}{R_2^2}(\mu-1)H_{i3}-\frac{R_1^2}{R_2^2}(\mu-1)H_{i2}-\frac{r_1^2}{R_2^2}(\mu-1)H_{i1}=-(\mu-1)H_e\\
&(\mu-1)H_{i6}-(\mu+1)H_{i5}-\frac{R_2^2}{r_3^2}(\mu-1)H_{i4}-\frac{r_2^2}{r_3^2}(\mu-1)H_{i3}-\frac{R_1^2}{r_3^2}(\mu-1)H_{i2}-\frac{r_1^2}{r_3^2}(\mu-1)H_{i1}=-(\mu-1)H_e\\
&(\mu+1)H_{i6}-\frac{r_3^2}{R_3^2}(\mu-1)H_{i5}-\frac{R_2^2}{R_3^2}(\mu-1)H_{i4}-\frac{r_2^2}{R_3^2}(\mu-1)H_{i3}-\frac{R_1^2}{R_3^2}(\mu-1)H_{i2}-\frac{r_1^2}{R_3^2}(\mu-1)H_{i1}=-(\mu-1)H_e
\end{aligned}
$$

We have now two sets of six linear equations in the six quantities $H_{i1} \cdots H_{i6}$, one set corresponding to the spherical shells and the other to the cylindrical shells. We have sufficient equations in each case to enable us to evaluate $H_{i1} \cdots H_{i6}$ in terms of the geometrical data, the permeability and the impressed field.

It will be convenient to treat these equations by means of determinants, and the method used in treating the spherical problem only will be given; the cylindrical problem is subject to a treatment mathematically analogous to that indicated for the spherical problem.

Referring to the set of equations applying to spherical shells, let the determinant formed by taking in order the coefficients of the H's in the left-hand member of the first equation for the elements of its first row, the coefficients, in order, of the H's in the left-hand member of the second equation for the elements of its second row and so on be called Δ. Let the determinant which is equal to Δ except in the first column, where in place of the elements found in the first column is put in the place of each element the common right-hand member of the system of equations namely—$(\mu - 1)H_e$, be called $\Delta 1$. Let $\Delta 2$, $\Delta 3$, $\Delta 4$, $\Delta 5$ and $\Delta 6$ be defined in an analogous way. The determinants thus defined are seen to be of the sixth order.

We shall have then according to Leibnitz's rule for the solution of simultaneous linear equations the following values of $H_{i1} \cdots H_{i6}$ pertaining to the spherical shells

$$H_{i1} = \frac{\Delta 1}{\Delta}$$

$$\vdots \quad \vdots \quad \vdots \, .$$

$$H_{i6} = \frac{\Delta 6}{\Delta}$$

These are the values to be substituted in the equation

$$H_{i1} = H_e + H_{i6} + \cdots + H_{i1}$$

and it will be remembered H_{i1} stands for the field within the innermost spherical shell and H_e for the impressed field. From the method of formation of $\Delta 1 \cdots \Delta 6$ it will be seen that each of these determinants contains H_e as a factor, and the shielding ratio $g = \dfrac{H_e}{H_a}$

is found from the equation just given. Substituting in this equation the values found for $H_{i1} \cdots H_{i6}$ we get

$$H_{i1} = H_e \left\{ 1 + \frac{1}{H_e}\left[\frac{\varDelta 1 + \varDelta 2 \cdots \varDelta 6}{\varDelta}\right] \right\}$$

and therefore

$$g = \frac{H_e}{H_{i1}} = \frac{He\varDelta.}{\varDelta H_e + \varDelta 1 + \varDelta 2 + \cdots + \varDelta 6}.$$

This then expressed symbolically in determinantal form is the result sought. It remains now to expand the $\varDelta$'s and reduce the somewhat complicated resultant expression. It will be sufficient to illustrate the method used in expanding the various determinants to consider one of them only, say $\varDelta$.

It is in the ordinary notation

$$\begin{vmatrix} (\mu-1), & (\mu-1), & (\mu-1), & (\mu-1), & (\mu-1), & -(2\mu+1) \\ (\mu-1), & (\mu-1), & (\mu-1), & (\mu-1), & (\mu+2), & -2(\mu-1)\frac{r_1^3}{R_1^3} \\ (\mu-1), & (\mu-1), & (\mu-1), & -(2\mu+1), & -2(\mu-1)\frac{R_1^3}{r_2^3}, & -2(\mu-1)\frac{r_1^3}{r_2^3} \\ (\mu-1), & (\mu-1), & (\mu+2), & -2(\mu-1)\frac{r_2^3}{R_2^3}, & -2(\mu-1)\frac{R_1^3}{R_2^3}, & -2(\mu-1)\frac{r_1^3}{R_2^3} \\ (\mu-1), & -(2\mu+1), & -2(\mu-1)\frac{R_2^3}{r_3^3}, & -2(\mu-1)\frac{r_2^3}{r_3^3}, & -2(\mu-1)\frac{R_1^3}{r_3^3}, & -2(\mu-1)\frac{r_1^3}{r_3^3} \\ (\mu+2), & -2(\mu-1)\frac{r_3^3}{R_3^3}, & -2(\mu-1)\frac{R_2^3}{R_3^3}, & -2(\mu-1)\frac{r_2^3}{R_3^3}, & -2(\mu-1)\frac{R_1^3}{R_3^3}, & -2(\mu-1)\frac{r_1^3}{R_3^3} \end{vmatrix}$$

Multiply the sixth row by R_3^3, the fifth by r_3^3, the fourth by R_2^3, the third by r_2^3, the second by R_1^3, the first by r_1^3. To keep the value of the determinant unchanged we must multiply the resulting form by $\frac{1}{R_3^3 \ldots r_1^3}$. Now subtract the fifth row from the sixth, the fourth from the fifth and so on down to the first row. These operations have not changed the value of the determinant. Now in the resultant form subtract the second column from the first, the third from the second and so on up to the sixth. Divide the sixth row of the resultant expression by R_3^3, the fifth by r_3^3 and so on down to the first row. To keep the value of the determinant unchanged multiply the resulting form by $R_3^3 \ldots r_1^3$. Employing the p and m notation explained on page 195 we may write the resulting form of the determinant Δ as follows :

$$\Delta = \begin{vmatrix} 0 & 0 & 0 & 0 & 3\mu & -(2\mu+1) \\ 0 & 0 & 0 & -3 & (\mu+2)m_1 & 3p_1 \\ 0 & 0 & 3\mu & -(2\mu+1)m_{12} & -3\mu p_{12} & 0 \\ 0 & -3 & (\mu+2)m_2 & 3p_2 & 0 & 0 \\ 3\mu & -(2\mu+1)m_{23} & -3\mu p_{23} & 0 & 0 & 0 \\ (\mu+2)m_3 & 3p_3 & 0 & 0 & 0 & 0 \end{vmatrix}$$

As the determinant now stands Laplace's Method of development will be found convenient. By this method the determinant Δ is at once seen to be equal an expression with two terms, each of which is the product of two determinants of the third order. Using the "criss-cross" method applicable to determinants of the third order the expansion of these determinants is easily obtained and thus the expansion of Δ. In a similar way the expansions of $\Delta 1 \ldots \Delta 6$ may be obtained.

We have now to substitute the values found for Δ, $\Delta 1 \ldots \Delta 6$ in the expression

$$g = \frac{\Delta . H_e}{\Delta H_e + \Delta 1 + \Delta 2 + \ldots + \Delta 6}.$$

Assuming this to have been done and the proper simplifications and rearrangement of the terms to have been accomplished we get

$$\begin{aligned} g - 1 = \tfrac{2}{9} \frac{(\mu-1)^2}{\mu} \Big\{ & (1 - p_1 p_2 p_3) \\ (A) \qquad & + \tfrac{1}{81} \frac{(2\mu+1)^2(\mu+2)^2}{\mu^2} m_1 m_{12} m_2 m_{23} m_3 \\ & + \tfrac{1}{9} \frac{(2\mu+1)(\mu+2)}{\mu} \left[(m_1 m_3 + m_1 m_2 - m_1 m_2 m_3) m_{12} \right. \\ & \left. + (m_1 m_3 + m_2 m_3 - m_1 m_2 m_3) m_{23} - m_1 m_3 m_{12} m_{23} \right] \Big\}. \end{aligned}$$

A similar treatment of the fundamental equations for the cylindrical shells gives the corresponding formula

$$\begin{aligned}
g - 1 = \tfrac{1}{4}\frac{(\mu-1)^2}{\mu}\Bigg\{ & (1 - q_1q_2q_3) + \tfrac{1}{16}\frac{(\mu+1)^4}{\mu^2}\, n_1n_{12}n_2n_{23}n_3 \\
(B) \qquad & + \tfrac{1}{4}\frac{(\mu+1)^2}{\mu}\big[(n_1n_3 + n_1n_2 - n_1n_2n_3)\, n_{12} \\
& + (n_1n_3 + n_2n_3 - n_1n_2n_3)n_{23} - n_1n_3n_{12}n_{23}\big] \Bigg\}
\end{aligned}$$

Equations (A) and (B) are capable of transformation into the following forms:

$$\begin{aligned}
g = & \Bigg\{ \Big(\tfrac{2}{9}\frac{(\mu-1)^2}{\mu} m_1 + 1\Big)\Big(\tfrac{2}{9}\frac{(\mu-1)^2}{\mu} m_2 + 1\Big)\Big(\tfrac{2}{9}\frac{(\mu-1)^2}{\mu} m_3 + 1\Big) \Bigg\} \\
& + \tfrac{2}{9}\frac{(\mu-1)^2}{\mu}\cdot\frac{(2\mu+1)(\mu+2)}{9\mu}\Bigg\{ \frac{(2\mu+1)(\mu+2)}{9\mu} m_1m_2m_3p_{12}p_{23} \\
(A') \qquad & \\
& - \tfrac{2}{9}\frac{(\mu-1)^2}{\mu}(m_1m_2m_3p_{12} + m_1m_2m_3p_{23}) - (m_1m_2p_{12} + m_2m_3p_{23} \\
& + m_1m_3p_{12}p_{23}) \Bigg\}.
\end{aligned}$$

$$\begin{aligned}
g = & \Bigg\{ \Big(\tfrac{1}{4}\frac{(\mu-1)^2}{\mu} n_1 + 1\Big)\Big(\tfrac{1}{4}\frac{(\mu-1)^2}{\mu} n_2 + 1\Big)\Big(\tfrac{1}{4}\frac{(\mu-1)^2}{\mu} n_3 + 1\Big) \Bigg\} \\
(B') \quad & + \tfrac{1}{16}\frac{(\mu^2-1)^2}{\mu^2}\Bigg\{ \frac{(\mu+1)^2}{4\mu} n_1n_2n_3q_{12}q_{23} \\
& - \tfrac{1}{4}\frac{(\mu-1)^2}{\mu}(n_1n_2n_3q_{12} + n_1n_2n_3q_{23}) - (n_1n_2q_{12} + n_2n_3q_{23} + n_1n_3q_{12}q_{23}) \Bigg\}.
\end{aligned}$$

A few experimental determinations of the shielding ratio of three cylindrical shells made of transformer iron have furnished a satisfactory verification of the formula (B').

DISCUSSION OF THE EQUATIONS.

Consider first equations (A) and (B). g is seen to be a function of the geometrical quantities represented by the p's and the m's, the q's and the n's and of the permeability μ. In both (A) and (B) the expression for g is of the third degree in μ. In both g equals unity when

$\mu = 1$ and ∞ when $\mu = \infty$. [These remarks apply also to equations (A') and (B').] If any one of the three shells be allowed to vanish we have the case of two shells and the form of the resulting equations is seen to be independent of the shell which we choose to make vanish. Suppose in the case of three spherical shells we make the outer one vanish by causing the outer radius to become equal to the inner radius. Equation (A) then takes the form

$$g - 1 = \frac{2}{9}\frac{(\mu - 1)^2}{\mu}\left\{(1 - p_1 p_2) + \frac{(2\mu + 1)(\mu + 2)}{9\mu} m_1 m_2 m_{12}\right\}.$$

This is identically the equation given for two spherical shells by Professor du Bois[1] and a similar reduction for the cylindrical case would give the corresponding formula for two cylindrical shells. If we suppose the two outer spherical shells to vanish we obtain the well-known formula expressing the shielding effect of a single shell

$$g - 1 = \frac{2}{9}\frac{(\mu - 1)^2}{\mu}(1 - p_1).$$

Referring to the equations (A), (B), (A'), (B'), it is to be noticed that the subscripts 1 and 3 may be throughout interchanged without changing the equations; that is to say the formulæ are symmetrical with respect to the inner and outer shells.

Of course (A') and (B') are capable of discussion in a manner similar to that given above for (A) and (B), difference of form in the results occurring. The forms (A') and (B') are particularly adapted to a form of the discussion to be given now.

Suppose the outer shell to have so far expanded that there is no "magnetic interference" between it and the middle shell ($p_{23} = 0$, $m_{23} = 1$) and so far as to be also consistent with the condition that the middle shell may so expand as to cause the mutual interference between it and the inner shell to vanish ($p_{12} = 0$, $m_{12} = 1$) and yet the mutual interference between the middle and outer shell to remain zero. In this assumed expansion the ratio of the radii of each shell is supposed to remain constant. This means, of course, that R_3, r_3, R_2, r_2, become very large. The field within the outer shell may be now considered to act directly upon the middle shell as an impressed field; denote it by H_1. Likewise the field within the middle shell may be considered to act directly upon the inner shell as an impressed field; denote it by H_2. Denote the field within the inner shell by H_1. We have now

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[1] du Bois, Wied. Ann., 63, p. 353. 1898.

$$g = \frac{H_e}{H_3} \cdot \frac{H_3}{H_2} \cdot \frac{H_2}{H_1} = g_3 \, g_2 \, g_1$$

and have so expressed what Professor du Bois designates as a multiplication of shielding ratios, g_3, g_2, g_1 representing the shielding effect of the outer, middle and inner shell respectively against the field impressed upon each of them. We have then

$$g_3 = \frac{H_e}{H_3} = \tfrac{2}{9} \frac{(\mu - 1)^2}{\mu} m_3 + 1,$$

$$g_2 = \frac{H_3}{H_2} = \tfrac{2}{9} \frac{(\mu - 1)^2}{\mu} m_2 + 1,$$

$$g_1 = \frac{H_2}{H_1} = \tfrac{2}{9} \frac{(\mu - 1)^2}{\mu} m_1 + 1,$$

for it will be remembered that the form of the expression giving the shielding effect of a single spherical shell is $g = \tfrac{2}{9} \frac{(\mu - 1)^2}{\mu} m + 1$. Now in the expansion of the shells mentioned above it was specified that the ratio of the inner to the outer radius should remain constant for each shell. This means p_1, p_2 and p_3 and therefore m_1, m_2 and m_3 must also remain constant. So we may write equations (A') and (B') as follows:

$$(A'') \quad g = g_1 g_2 g_3 + \tfrac{2}{9} \frac{(\mu - 1)^2}{\mu} \frac{(2\mu + 1)(\mu + 2)}{9\mu} \left\{ \frac{(2\mu + 1)(\mu + 2)}{9\mu} \right. m_1 m_2 m_3 p_{12} p_{23} - \tfrac{2}{9} \frac{(\mu - 1)^2}{\mu} (m_1 m_2 m_3 p_{12} + m_1 m_2 m_3 p_{23}) \left. - (m_1 m_2 p_{12} + m_2 m_3 p_{23} + m_1 m_3 p_{12} p_{23}) \right\}$$

$$(B'') \quad g = g_1 g_2 g_3 + \tfrac{1}{16} \frac{(\mu^2 - 1)^2}{\mu^2} \left\{ \frac{(\mu + 1)^2}{4\mu} n_1 n_2 n_3 q_{12} q_{23} \right. - \tfrac{1}{4} \frac{(\mu - 1)^2}{\mu} (n_1 n_2 n_3 q_{12} + n_1 n_2 n_3 q_{23}) - (n_1 n_2 q_{12} + n_2 n_3 q_{23} \left. + n_2 n_3 q_{12} q_{23}) \right\}$$

(B'') being derived for the cylindrical shells in a manner analogous to that used for (A''). A consideration of (A'') and (B'') shows that the combined effect of the terms following $g_1g_2g_3$, is strongly subtractive, that is the shielding is much greater when the shells are very far removed from one another and as we have seen the shielding ratio in this case is equal to $g_1g_2g_3$. We may think then of the product $g_1g_2g_3$ as representing an "ideal" shielding and the terms following this product as representing the departure from this ideal case caused by the shells mutually interfering.

If μ is large, say > 100, (A) and (B) may be simplified as follows

$$(A_1)\quad \begin{aligned} g-1 = {}& \tfrac{2}{9}(\mu-2)\{(1-p_1p_2p_3)+\tfrac{4}{81}(\mu+1)(\mu \\ & +2)m_1m_{12}m_2m_{23}m_3+\tfrac{2}{9}(\mu+2)[(m_1m_3+m_1m_2 \\ & -m_1m_2m_3)m_{12}+(m_1m_3+m_2m_3-m_1m_2m_3)m_{23} \\ & -m_1m_3m_{12}m_{23}]\} \end{aligned}$$

$$(B_1)\quad \begin{aligned} g-1 = {}& \tfrac{1}{4}(\mu-2)\{(1-q_1q_2q_3)+\tfrac{1}{16}(\mu+2)^2n_1n_{12}n_2n_{23}n_3 \\ & +\tfrac{1}{4}(\mu+2)[(n_1n_3+n_1n_2-n_1n_2n_3)n_{12}+(n_1n_3+n_2n_3 \\ & -n_1n_2n_3)n_{23}-n_1n_3n_{12}n_{23}]\}. \end{aligned}$$

Similar approximate formulæ might be deduced from the equations (A'), (B'), (A''), (B'') and often prove useful.

GRAPHICAL DISCUSSION OF A PARTICULAR PROBLEM.

Professor Rücker has discussed analytically the question of maximum shielding, with reference to the weight of material used, for several particular problems. Among other things he has shown that if the radius a of a spherical shielded space be fixed and the amount of shielding material given the latter is best employed in the form of a *single* shell until a limit t is reached defined by $\frac{t}{a}=\frac{3}{2\mu}$ (approximately) where t denotes the thickness. So for a comparatively small shielded space, since μ for the best material reaches the initial value 300, it is only for extremely thin shells that the shielding with a single shell is, strictly speaking, most economical as regards weight. He has shown further that with two and three spherical shells when the permeability is great the arrangement with regard to minimum weight of material is best when the radii of the successive bounding surfaces are in geometrical progression, the ratio of the innermost to the outermost radius being supposed given. Professor Rücker's discussion is limited to spherical shells.

Professor du Bois has discussed the bi-lamellar case for both spherical and cylindrical shells when the innermost radius is given, and has found the best arrangement to be that for which the radial ratio of the air-gap is 1.5538 for cylindrical shells and 1.3815 for spherical shells; strictly speaking these results only hold for two thin similar shells (*i. e.*, of equal radial ratios); but the best radial ratios of the air space, as calculated for other less simple cases in actual practice, never come out much different from the above values; hence for designing purposes it may be roughly fixed between 1.3 and 1.4 for spheres, and between 1.5 and 1.6 for cylinders.

The following graphical discussion is introduced in the hope of showing in a manner convincing and easy to understand the advantages of lamination of the shielding material and not with the intent to give the most general possible discussion of the equations used in plotting the curves. At the same time it is hoped that the general explicit equations given above and the following illustration of their applicability to the plotting of shielding curves may be of use to those who may have occasion to design shielding apparatus.

We shall discuss here the shielding of single, bi-lamellar and tri-lamellar spherical and cylindrical shells under the supposition that the permeability of the shielding material is, in all cases, 202, which is but about two-thirds of that of the best material hithero tested, and under other conditions to be mentioned later.

Referring to Fig. 2, the curves shown relate to single spherical and cylindrical shells. The abscissæ represent the radial ratios of the shells; if the radius of the shielded space should be made equal to unity ($r = 1$) then the abscissæ would represent the successive values of the outer radius of the shell. The ordinates of curves 1 and 2 represent the shielding ratio, g. Curves 1 and 2 show how the shielding increases as the thickness of the shells increases. At first the gradient of the cylindrical curve is not quite so sharp as that of the spherical; later as the thickness increases it becomes greater and does not reach its asymptotic value so soon as in the spherical curve. Curve 1 reaches very nearly its asymptotic value when its outer radius is say three times the inner. The corresponding curve 2 for the hollow cylinder has not quite reached this point even when the outer radius has become five times the inner. These curves show clearly the futility of increasing the thickness of a single shielding shell beyond a certain point. Curves 1′ and 2′, corresponding to spherical and cylindrical shells respectively, represent, upon a scale quite arbitrary in each case as regards the ordinates, the way in which the shielding per unit weight of material varies as the thickness of the shells is increased.

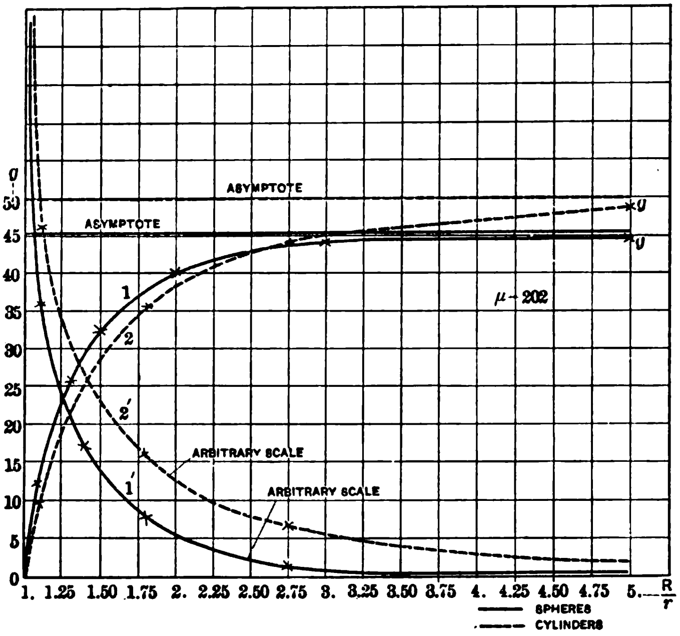

Fig. 2.

Consider now Fig. 3. It refers to the case of two concentric spherical shells, also to that of two concentric cylindrical shells. If r and R denote the innermost and outermost radii respectively of either system of shells, a condition imposed is that $\frac{R}{r}$ shall always equal five. The other conditions are that the two shells must be equal in thickness and that $\mu = 202$. If the radius of the shielded space be taken equal to unity ($r = 1$) then $R = 5$. The figures given as the abscissæ in the present case and in the case of three shells are to be considered on the supposition that $r = 1$. The independent variable is the thickness of the air-gap. It may vary between the limits 0 and 4. The upper of the two rows of abscissæ represents the varying air-gap thickness and the lower the corresponding thickness of the shells. The ordinates represent the shielding ratio g. Curves 1 and 2 are without reference to the weight and refer to spherical and cylindrical shell respectively. Curves 1′ and 2′ represent upon scales quite arbitrary as regards the ordinates the way in which the shielding per unit weight varies as the air-gap is increased or decreased.

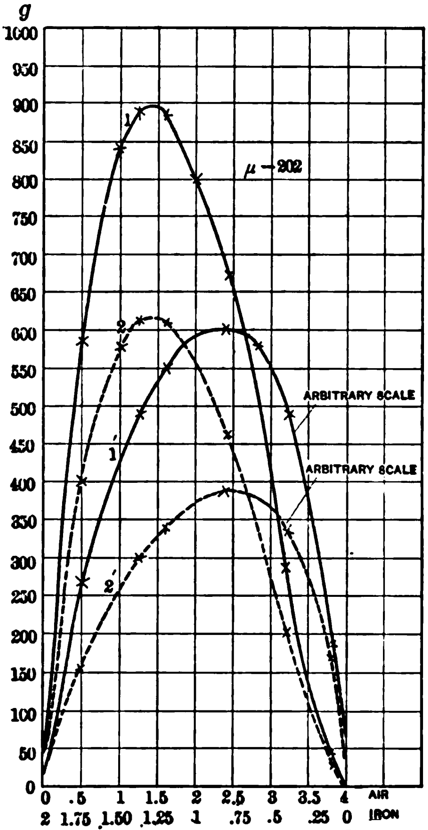

Fig. 3.

While graphically it is difficult to specify the exact position of the maximum points, yet it may be seen that the maxima for curves 1 and 2 occur for very nearly the same value of the air-gap distance and for that value for which the thickness of the shells and the airgap is the same. But the maxima for curves 1′ and 2′ occur much further to the right when the shells have become notably thinner; and approximately for the same value of the air-gap thickness and for that value which makes the air-gap thickness about three times the common thickness of the shells. By means of two shells arranged as specified above it would be possible, in the case of spherical shells, to reduce our impressed field to about $\frac{1}{900}$ of its original value within the space to be shielded. Had all of the available shielding space been filled up with iron we should have obtained but about $\frac{1}{20}$ as complete shielding and should have used about 1.3 as much iron, showing clearly the advantage of using air instead of iron in some parts of the shielding arrangement, and also how the addition of iron at the wrong place may be decidedly harmful.

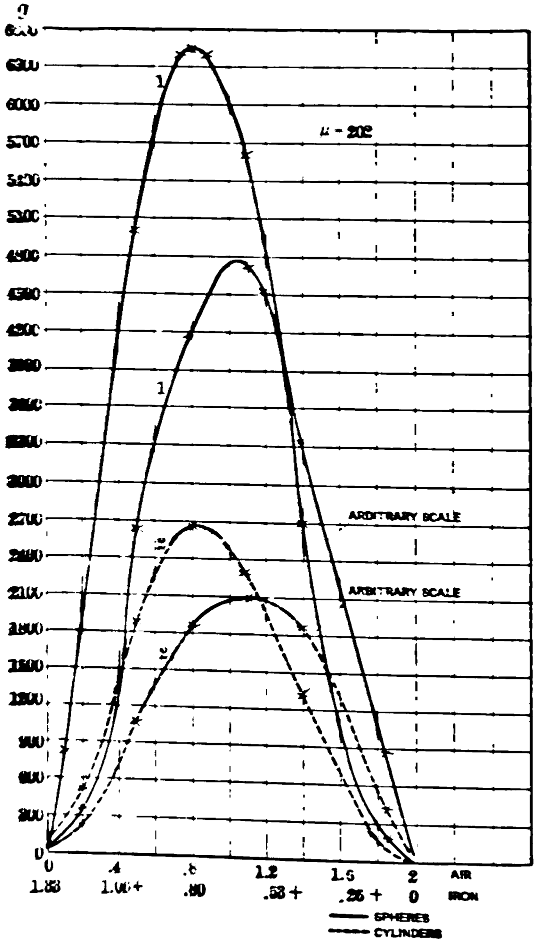

Fig. 4.

In Fig. 4 we have represented the case of three spherical shells and also of three cylindrical shells under conditions quite similar to those laid down for the bi-lamellar shells. The conditions are: that $\mu = 202$; that the shells have always a common thickness; that the two air-gaps have the same thickness; that $r = 1$, $R = 5$, r and R denoting the innermost and outermost radius respectively of the shells in both cases (spherical and cylindrical). Curves 1 and 2 refer, without reference to the weight, to the spherical and cylindrical shells respectively. Curves 1′ and 2′ are the corresponding curves representing the variation in the shielding per unit weight. The upper of the two rows of abscissæ represents the common thickness of the two air-gaps and it is the thickness which has been taken as the independent variable. The second row represents the corresponding common thickness of the three shells in both cases (spherical and cylindrical). Curves 1 and 2 reach a maximum very nearly simultaneously and for that value of the air-gap thickness at which very approximately, at any rate, the common thickness of the air-gaps equals that of the shells. Curves 1′ and 2′ whose ordinates are drawn upon independent arbitrary scales reach their maximum points nearly simultaneously and for a value of the air-gap thickness, which makes the thickness of each air-gap about twice the thickness of each shell. With three spherical shells as specified above it would be possible then to reduce an impressed field to about $\frac{1}{6400}$ of its original value within the shielded space. Had all of the available shielding space been filled up with iron we should have obtained but about $\frac{1}{150}$ as complete shielding and should have used about 1.5 as much iron. The advantages of an extended lamination of the shielding material are hereby made very obvious.

As was mentioned above, this graphic discussion is intended more as an illustrative problem than as one which would allow of the deduction of practical rules to cover the most probable conditions which might arise in practice.

In a joint paper by Professor du Bois and the writer the results of some experimental investigations with three cylindrical shells will be published.

I have to express my cordial thanks to Professor du Bois for his kind assistance during the development of the foregoing discussion.

Berlin, April 15, 1899.

A NEW RESPIRATION CALORIMETER AND EXPERIMENTS ON THE CONSERVATION OF ENERGY IN THE HUMAN BODY, II.

W. O. Atwater and E. B. Rosa.

III. Tests of the Apparatus as a Calorimeter.

TWO series of test experiments were instituted to ascertain whether the calorimeter would accurately measure a given quantity of heat generated within the respiration chamber. In the first series an electric current of measured strength flowing through a known resistance generated in a given time a quantity of heat which could be readily calculated. As there was no circulation of air and no water vapor produced in the apparatus, it was used at the best advantage simply as a calorimeter. In the second series alcohol was burned within the chamber, a current of air circulating through it supplying the necessary oxygen and carrying away the products of combustion. Here the complications arising from a ventilating current of air and the production of a large amount of moisture were introduced. Evidently, if the calorimeter will accurately measure a known quantity of heat generated in these two ways, it may be employed with confidence to measure the heat generated by a living subject within the chamber. In the latter case, however, we can not expect as high a degree of accuracy as in the former, for the rate of evolution of heat is more variable and the movement of the person or animal about the respiration chamber introduces disturbances which will prevent the highest degree of accuracy in the measurements; and furthermore the errors due to physiological causes may be considerable.

Electrical Test Experiments.

The arrangement of the apparatus in the electrical tests is shown in Fig. 13. The current from the 220-volt mains passes into the respiration chamber through a variable resistance, *RS*, a Weston ammeter, *Am*, and two copper voltameters, *Vm*, where it passes through *PQ*, a coil of German-silver wire No. 30, having a resistance of about 100 ohms. A Kelvin balance in series with a known resistance, R_1, in oil, and a Weston voltmeter in parallel with them are connected to the points *a*, *b*, and indicate the fall of potential through the resistance, *PQ*. Usually an assistant keeps the current constant by adjusting the resistance, *RS*. The current is given with considerable accuracy by the Weston ammeter, which in the first test experiment was used alone. But when the copper voltameters were employed the values for the current obtained with them were used.

Two test experiments were made on March 20 and 25, 1897, which gave very satisfactory results. A third on March 26th gave slightly more concordant results than either of the others and will be here given in some detail, in order to illustrate the operation of the calorimeter as well as to show its accuracy.

A sample page from the record of the test of March 26th, is given below. It gives the observations of one hour.

In the first column the time is recorded. In the second column are the deflections in the scale divisions in the thermo-electric circuit (No. 1) of the walls of the respiration chamber. Numbers at the left side of the column indicate deflections to the left and show that the inner chamber is a little cooler than air space *B*. At 5.10, the air in *B* having been cooled slightly, the deflection is zero. Then it becomes positive for a few minutes and the air in *B* requires slight warming. The deflections during the hour are on the average less than one division and indicate an average difference of temperature of about 0.004° C. But this difference is part of the time positive and part of the time negative, so that the algebraic average is only one-fourth of one division to the right, indicating that the inner chamber was on the average for the hour 0.0018° C. warmer than the surrounding space. By carrying these differences along from page to page the adjustments are made so that the differences cancel each other, and there is no correction to apply for flow of heat throught the walls for the whole experiment.

One division deflection corresponds, as already stated, to 0.007°

TABLE I.

Sample page from record of calorimetric observations of experiment of March 26, 1897.

Time, p. m.	Inner wall.		Water circuit.			Inside temp. No. 5.	Remarks.
	No. 1.	Parts.	T_1, T_2.	$T_2 - T_1$.	Bridge No. 6.		
h. m.			°*C.*	°*C.*	°*C.*	°*C.*	
	−3½	+2					
5 00	½	+1			12.80	22.61	
02	0	0			12.80	22.61	
04	0	−1½			12.79	22.61	1st 10 kilos at 5.04.00.
06	0				12.80	22.62	Av. bridge read. 12.73.
08	1				12.87	22.62	12.73−.27=12.46°.
10	0				12.85	22.62	12.46°×10=124.6
12	1½		15.97		12.84	22.61	calories.
14	1½		3.42	12.55	12.78	22.62	
16	1½				12.72	22.62	
18	1½				12.80½	22.62	
20	1	+1			12.81	22.62	
22	½	+1½			12.84½	22.62	
24	0	−1			12.83½	22.62	
26	0	0			12.86	22.62	
28	0		15.93		12.76	22.63	
30	½		3.44		12.77	22.63	
32	0				12.87½	22.63	
34	½			12.49	12.77	22.64	
36	1				12.80½	22.64	
38	0				12.81	22.64	
40	1				12.81	22.64	
42	½		15.94		12.81	22.64	
44	½	0	3.43	12.51	12.77	22.64	
46	0	−½			12.78	22.65	2d 10 kilos at 5.47.45.
48	1	0			12.79	22.65	Av. bridge read. 12.81.
50	½	−½			12.86	22.65	12.81−.27=12.54°.
52	0				12.83	22.65	12.54°×10=125.4
54	½				12.79	22.65	calories.
56	0		16.04		12.81	22.64	
58	0		3.47	12.57	12.85	22.64	
	7 +11½						

C. difference of temperature, and with this difference of temperature 4 small calories per minute, or 0.24 of a large calorie per hour, flow through the walls of the chamber. This was determined in the following manner: A current of 0.17 ampères was passed through a resistance within the calorimeter for four hours. The fall of potential was 43 volts, giving 7.3 watts or about 104 small calories per minute. The temperature of the chamber rose a little at first, but was constant during the latter part of the experiment, showing that all of the heat was escaping through the walls. The deflection produced by the thermo-electric elements of circuit No. 1 was about 25 divisions, remaining substantially constant for some time before the end of the experiment. This gives 4 small calories per minute per scale division as the amount of heat flowing through the walls of the calorimeter. This may be called the radiation constant.

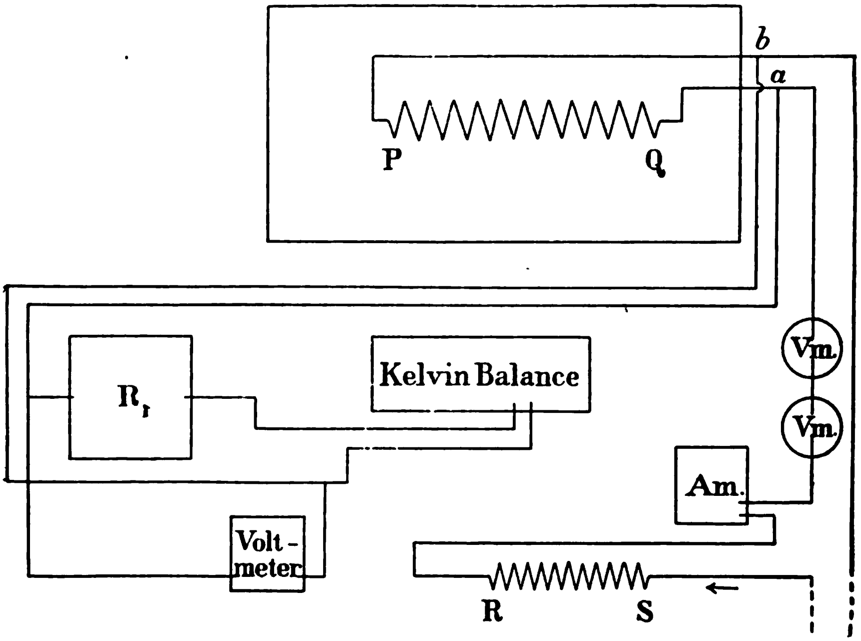

Fig. 13.—Arrangement of the electric apparatus for generating heat in the test experiments.

For an average deflection of one-fourth of one division the rate at which heat would be escaping from the interior of the calorimeter would be 0.06 calorie per hour. The amount of heat generated and carried away from the calorimeter was, in this experiment, 74 calories per hour. Thus the amount of heat passing through the walls is only $0.060 \div 74 = 0.0008$ of the total, or one-twelfth of 1 per cent., and would not be an appreciable error if uncorrected. It is, however, subsequently recovered by keeping the deflection slightly negative, as already explained.

In the third column are recorded the deflections observed when parts of the thermo-electric circuits are separately joined to the galvanometer, the four numbers being for the top, upper, lower and bottom sections, respectively. These deflections are all small and make more certain the balance indicated by zero deflection of the entire system in series.

In the fourth column are recorded the readings of the two mercury thermometers, *G* and *H* (Fig. 6), and the fifth column shows their difference $(T_2 - T_1)$, which is the gain in temperature of the water flowing through the absorbing pipes. The sixth column shows the readings of bridge No. 6 for the same difference of temperature.

The bridge does not give the exact difference of temperature of the two coils, which are immersed in the water, its readings being always too high. However, it correctly indicates the variations of temperature between the thermometer readings, and thus interpolations between the observed thermometer readings can be made. In actual practice the average of the bridge readings is reduced by the average difference between thermometer and bridge. For example 10 kilograms ran out during the period of forty-four minutes from 5.04 to 5.48 p. m. The average of the bridge readings for the forty-four minutes, during which the 10 kilograms of water were passing through the absorbers, was 12.81. The correction to be subtracted to reduce the bridge readings to degrees centigrade is 0.27. This gives an average difference of 12.54°, and hence 125.4 calories of heat were brought away in these forty-four minutes. In the same way the difference of temperature for each 10 kilograms is determined. A tabular statement of the heat measured during the entire experiment from 4.20 p. m. to 11.37 p. m. is given in the table on next page.

If a calorie be defined as the amount of heat required to raise 1 kilogram of water 1° C., at 15° C. it will be equivalent to a definite number of ergs, which must be determined by experiment. The value given by E. H. Griffiths,[1] as the result of his elaborate work

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[1] Phil. Trans. Royal Society, A, 1893. Proc. Roy. Soc., vol. 55.

TABLE II.

Summary of heat measurements, March 26, 1897.

Time.	(*a*) Duration of period.	Calorimetric measurements.				Electrical measurements.				Temperature of the inside by the copper thermometer. No. 5.
		(*b*) Water.	(*c*) Difference in temperature by bridge No. 6.	(*d*) Corrected difference in temperature, *c*—0.27.	(*e*) Amount of heat, *b* × *d*.	*E* Voltage on coil.	*I* Current through coil.	*W* Watts, *E* × *I*	(*k*) Calories, *W* × *a* ×.2378.	
h. m. s.	*Secs.*	*Kilos.*	°*C.*	°*C.*	*Cals.*		*Amps.*			°*C.*
4 20 20	......	....	......	......	......	......	......	....	.....	22.63
5 04 00	2,620	10	12.73	12.46	124.60	147.8	1.363	201.5	125.54	22.61
5 47 45	2,625	10	12.81	12.54	125.40	147.8	1.363	201.5	125.78	22.65
6 31 45	2,640	10	12.92	12.65	126.50	147.8	1.363	201.5	126.50	22.67
7 15 27	2,622	10	12.77	12.50	125.00	146.0	1.350	197.1	122.89	22.57
7 59 10	2,623	10	12.80	12.53	125.30	147.8	1.363	201.5	125.68	22.59
8 42 40	2,610	10	12.79	12.52	125.20	147.8	1.363	201.5	125.06	22.60
9 26 15	2,615	10	12.78	12.51	125.10	147.8	1.363	201.5	125.30	22.60
10 09 50	2,615	10	12.68	12.41	124.10	147.8	1.363	201.5	125.30	22.64
10 53 30	2,620	10	12.82	12.55	125.50	147.8	1.363	201.5	125.54	22.62
11 37 10	2,620	10	12.81	12.54	125.40	147.8	1.363	201.5	125.54	22.61
					1,252.10	Heat generated by electric current.... = 1,253.13				
Capacity correction for 0.02°...... =					—1.2					
Heat measured by calorimeter..... =					1,250.9					

on the determination of the relation between the electric units and those of heat is,

$$J = 4.1982 \times 10^7 \text{ ergs, at } 15^\circ \text{ C.}$$

The average temperature of the water in the absorbers in our experiments was in every case between 8° and 10° C. The specific heat of water[1] at 10° C. is 1.0019[2] times its value at 15° C., and hence it requires 1.0019 times as much heat to raise 1 kilogram one degree at 10° C. as at 15° C., so that

$$J = 4.2062 \times 10^7 \text{ ergs, at } 10^\circ \text{ C.}$$

This value is practically the same as that given by Professor Rowland[3] for the mechanical equivalent of heat at 8° C., and so appears to be the most nearly correct for our use.

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[1] Bartoli and Stracciati, Boll. mens. dell'Acc. Gioenia, 18, Apr. 26, 1891. Preston's Theory of Heat, p. 265.

[2] As estimated from the figures of the table on p. 231—*i. e.*, 1.0029 ÷ 1.0010 = 1.0019.

[3] Proc. Am. Acad. Sci., 15 (1879), p. 75.

A recent recalculation of Rowland's value of the mechanical equivalent of heat[1] gives 4.200×10^7, at 8° C., and it has been suggested that the difference between this value and Griffith's is accounted for by the uncertainty in the value of the ampère, but as we use the same ampère as Griffith did we should use his value of *J*.

The energy of the electric current expressed in ergs being

$$CEt \times 10^7,$$

when expressed in calories becomes

$$\frac{CEt \times 10^7}{4.2062 \times 10^7} = CEt \times 0.23775.$$

Referring to the above table, it will be seen that the total amount of heat generated by the current is 1253.13 calories. The amount measured is 1252.10 calories, less than that produced by the electric current by 1.03 calories. During the experiment the temperature of the chamber fell from 22°.63 to 22°.61. The capacity for heat of the apparatus is equivalent to that of 60 kilograms of water —that is, it requires 60 calories to raise the temperature 1°. This was determined in the following manner:

The calorimeter was held at a constant temperature (23°.8) for several hours. A current of 1.70 ampères, at 33 volts, was then passed through it for two hours. The temperature slowly rose and, keeping the deflection of the No. 1 thermo-electric circuit zero, no heat was allowed to pass through the walls. At the end of this time the current was stopped and the calorimeter came to a constant temperature at 25°.4. This rise in temperature was thus 1°.6. The amount of heat generated by the electric current was 96.2 calories, or 60.1 calories per degree. Other determinations gave similar results and the round number 60 was taken as a sufficiently exact value of the thermal capacity of the apparatus.

In the above case, where the temperature fell 0°.02, there will be .02 × 60 = 1.2 calories which are absorbed and measured in addition to the heat from the current. Applying this correction, we have for the measured amount of heat produced by the current 1252.10 − 1.2 = 1250.9 calories, which is 2.2 calories, or 0.18 per cent, less than indicated by the electrical determination.

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[1] Phil. Mag. [illegible]

The current in the above experiment was maintained constant, and the voltage measured by the Kelvin balance. About 7 p. m. the voltage on the mains was lower than usual for some minutes, and the variable portion of the resistance was insufficient to maintain the voltage on the coil. Consequently the balance was set at a lower point and the current kept constant at this smaller value until it could be restored to its former value. This makes the average voltage and current for the fourth period smaller than the others. The totals show that the amount measured was 2.2 calories, or 0.18 per cent. too small. This, however, assumes that the measurements of temperature at the beginning and the end are accurate and that the temperature of the whole apparatus is the same as that of the air. An error of a few hundredths of a degree might throw the difference in the opposite direction. Since this change is so slight, and as all the measurements of the temperature are presumably equally reliable, it would be more accurate if the average of all was used in obtaining the capacity correction. This can be done by taking different portions of the experiment and applying the capacity correction to each portion; the average of the errors from the different portions will give a more probable value of the error of the experiment. The above experiment is thus discussed in Table III.

Table III.

Discussion of experiment of March 26, 1897.

Periods.	(*e*) Heat as measured.	Temperature No. 5.			(*i*) Capacity correction, *h* × 60.	(*j*) Corrected heat, *e* + *i*.	(*k*) Heat put in by the electric current.	Error in heat measurement, *j* − *k*.
		(*f*) At the beginning.	(*g*) At the end.	(*h*) Change, *g* − *f*.				
	Cals.	°	°	°	*Cals.*	*Cals.*	*Cals.*	*Cals.*
1-6........	752.0	22.63	22.60	−0.03	−1.8	750.2	751.5	−1.3
2-7......	752.5	22.61	22.60	−0.01	−0.6	751.9	751.1	+0.8
3-8.......	751.2	22.65	22.64	−0.01	−0.6	750.6	750.6	0.0
4-9........	750.2	22.67	22.62	−0.05	−3.0	747.2	749.6	−2.4
5-10.......	750.6	22.57	22.61	+0.04	+2.4	753.0	752.2	+0.8
					Sum	3,752.9	3,755.0	−2.1
					Avg.....	750.6	751.0	−0.4
	$\frac{0.4}{750.6}$ = 0.053 per cent.							

In the first column is indicated the number of periods corresponding to the section of the experiment taken for discussion. In the second, (*c*) is the heat measured during the same period—*i. e.*, the sum of the values in column (*c*), Table II. The next three columns show the change in temperature of the apparatus, and the necessary correction is given in the sixth column. Applying these corrections to the values in the second column gives the corrected heat in (*j*). The values of the heat generated, (*k*), are obtained by adding those in column (*k*), Table II. The last column gives the differences between the heat generated and that measured, and the average of these shows the error of the apparatus. In this experiment the error is about 0.05 per cent., which is as close as the electrical energy could be measured.

Another test of a somewhat different nature was made April 30th. A small incandescent electric lamp, rated at 4 candle power, but burned low, taking 4.21 watts, or 1 small calorie per second, was placed within the chamber and the heat given off was measured. The heat being so small it was necessary to have the water enter at a temperature nearly as high as the air inside, and to flow in a very slow stream. The results of six hours' run are given in Table IV.

TABLE IV.

Experiment with 4-candle power electric lamp—Summary of heat measurements, April 30, 1897.

	Calorimetric measurements.					Electrical measurements.				
Time.	Duration of period.	Water	Difference in temperature by bridge No. 1.	Corrected difference in temperature.	Amount of heat.	Voltage on lamp.	Current through lamp.	Watts	Calories	Temperature of the inside by the copper thermometer No. 5.
h. m. s.	Sec.	[illegible]	[illegible]	[illegible]	Cal.		Amp.			[illegible]
9 50 [illegible]	.....	...	.....	...	...	.....	...	....	.....	[illegible]
[illegible]	945	1	[illegible]	1.73	[illegible]	5.61	.75	4.21	[illegible]	[illegible]
[illegible]	[illegible]	2	[illegible]	[illegible]	2.52	5.61	.75	4.21	[illegible]	[illegible]
[illegible]	[illegible]	1	[illegible]	[illegible]	[illegible]	5.61	.75	4.21	[illegible]	[illegible]
[illegible]	[illegible]	3	[illegible]	[illegible]	[illegible]	5.61	.75	4.21	[illegible]	[illegible]
[illegible]	[illegible]	4	[illegible]	.88	[illegible]	5.61	.75	4.21	[illegible]	[illegible]
[illegible]	[illegible]	2	[illegible]	[illegible]	[illegible]	5.61	.75	4.21	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	5.61	.75	4.21	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	5.61	.75	4.21	[illegible]	[illegible]

Heat measured .... = [illegible] Heat generated .... = [illegible]

It will be seen that the measurement is very close. There is apparently a small capacity correction, which if applied, makes the error in the opposite direction. It is so small, however, that it is impossible to say with certainty how much the correction should be.

As this amount of heat is only about 1 per cent. of the maximum which the apparatus will measure and about 3 per cent. of the heat usually measured, it is evident that there can be no appreciable loss or gain of heat by radiation or otherwise. We therefore believe that this calorimeter, with proper manipulation, will measure heat as accurately as it is possible to measure it by any other form of calorimeter.

ALCOHOL TEST EXPERIMENTS.

The electrical tests above described demonstrate the accuracy of the apparatus as a calorimeter when the evolution of heat inside the chamber is reasonably uniform, and there is no current of air passing through and no vaporization of water within the chamber. In experiments with men, however, the development of heat is less uniform; furthermore, a current of air is passing through the chamber, water and carbon dioxid are produced, and water is vaporized within it. In these experiments the principal measurements made with the aid of the apparatus are the amounts of water and carbon dioxid produced by the man and the heat given off from his body. In experiments where the external muscular work has been considerable the latter has been transformed into electrical energy, measured, and transformed into heat. This heat has been measured with that given off directly by the body. The crucial test of the accuracy of the determinations of carbon dioxid, water and heat must be made in test experiments under circumstances closely similar to those of the actual experiments with men. When ethyl alcohol is burned in air carbon dioxid, water and heat are produced. If known quantities of alcohol be burned inside the chamber while a current of air is passing through, the conditions will approach very closely those of an experiment with man. To make these experiments accurate, the amount of alcohol burned must be exactly known and the combustion must be complete.

Several series of experiments were made in which alcohol was burned inside the chamber. The rate of flow of the ventilating current, the method of analysis of the incoming and outgoing air, and the method of measurement of heat were the same as if a man were inside the chamber.

1. COMPOSITION OF ALCOHOL USED—SPECIFIC GRAVITY.

In some of the tests here described we have used pure ethyl alcohol kindly furnished by Dr. Edward R. Squibb, but for the larger part of our work, including the alcohol tests with the respiration calorimeter, a high grade of commercial alcohol was used instead of the so-called absolute alcohol, because it can be more easily stored, weighed, and measured without change of water content. The specific gravity of this alcohol was determined at the time of each experiment. The variations in the different samples were small, the proportion of alcohol being not far from 90.5 per cent. by weight. From the percentage of alcohol present the amount of carbon dioxid and water that would be formed by complete oxidation is readily calculated as follows: Taking the atomic weight of hydrogen as 1, that of oxygen[1] is here taken as 15.88, and carbon 11.91. With these atomic weights the molecular weight of alcohol, C_2H_6O, is 45.70, of H_2O is 17.88 and of CO_2 is 43.67. The reaction in the combustion of alcohol is $C_2H_6O + O_6 = 2CO_2 + 3H_2O$. By molecular weights, $45.70 + 95.28 = 87.34 + 53.64$, or 1 gm. alcohol + 2.0848 gm. oxygen = 1.9110 gms. CO_2 + 1.1737 H_2O. Hence one gram of ethyl alcohol (C_2H_6O) will yield on oxidation 1.9110 grams of carbon dioxid and 1.1737 grams of water. One gram of a mixture of alcohol and water containing 90.77 per cent. absolute alcohol (the strength of that used in the first three tests) will give 1.9110 × 0.9077, or 1.7346 grams of carbon dioxid, and 1.1737 × 0.9077, or 1.0654 grams of water from the combustion of the ethyl alcohol in addition to the 0.0923 grams of water present in the mixture, making a total of 1.1577 grams of water from 1 gram of the alcohol mixture used. The estimates of the quantity of heat produced by the oxidation of a gram of each of the different specimens of alcohol are made by use of the factor

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[1] F. W. Clarke, Journal of Am. Chem. Soc., March, 1898. Richards, Am. Chem. Journal, p. 534, July, 1898

7.067 calories for the heat of the combustion of 1 gram pure ethyl alcohol as explained beyond.

2. HEAT OF COMBUSTION OF ALCOHOL—DETERMINATION WITH THE BOMB CALORIMETER.

The heat of combustion of the alcohol used in the tests was determined by the bomb calorimeter. This well known instrument[1] which has been used in determining the heats of combustion of the food and excreta in the experiments with a human subject, is illustrated in section in Fig. 14. The so-called bomb is a steel cylinder, about 12 cm. deep and 6 cm. average interior diameter, the wall being about 0.6 cm. thick. A groove in the cover contains a lead gasket, and when the outer ring or collar is screwed down with a heavy spanner, the bomb is closed perfectly tight, even for pressures of 30 to 40 atmospheres. The cover is provided with a neck *D*. The valve screw *F* passes through a second screw *E*, and at the bottom of *E* is a lead packing *L*. The pressure of *E* upon *L* makes a perfectly tight joint about the bottom of *F*. Oxygen is admitted to the bomb through a narrow passage extending from *G* to the bottom of *F*, and then directly downward to the interior of the cylinder. This passage is closed when *F* is screwed down. The bomb is fitted with a platinum cup, spun from a single piece of platinum, which serves as lining, and the cover is also lined with platinum. This effectually prevents any oxidation of the bomb itself by the compressed oxygen during the combustion. Two platinum rods *H*, *I*, serve to hold a platinum capsule *O*, containing the substance to be burned, and also to conduct an electric current which, flowing through the fine iron wire over the capsule, fuses the wire and so ignites the substance to be burned.

The bomb is placed in a cylinder of brittania metal, 12 cm. in diameter and 22 cm. high, and about 1700 c.c. of water added, the whole being surrounded by two concentric cylinders of "indurated fiber," in the manner shown in the figure. A stirrer *SS*, is driven at a uniform speed by a small electric motor. The thermometer is graduated to one-hundredth of a degree, and read with a magnifying glass to thousandths. Those here used were made by Fuess of Berlin and calibrated at the Physikalisch-technische Reichsanstalt. Oxygen is supplied by the S. S. White Dental Mfg. Co., New York in cylinders about 125 cms. long and 18 cm. in diameter, at a pressure of 200 atmospheres. It is admitted to the bomb until

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[1] See description by W. O. Atwater in Bulletin 21 of the Office of Experiment Stations of the U. S. Department of Agriculture and by W. O. Atwater and O. S. Blakeslee in report of Storrs (Conn.) Agricultural Experiment Station for 1897.

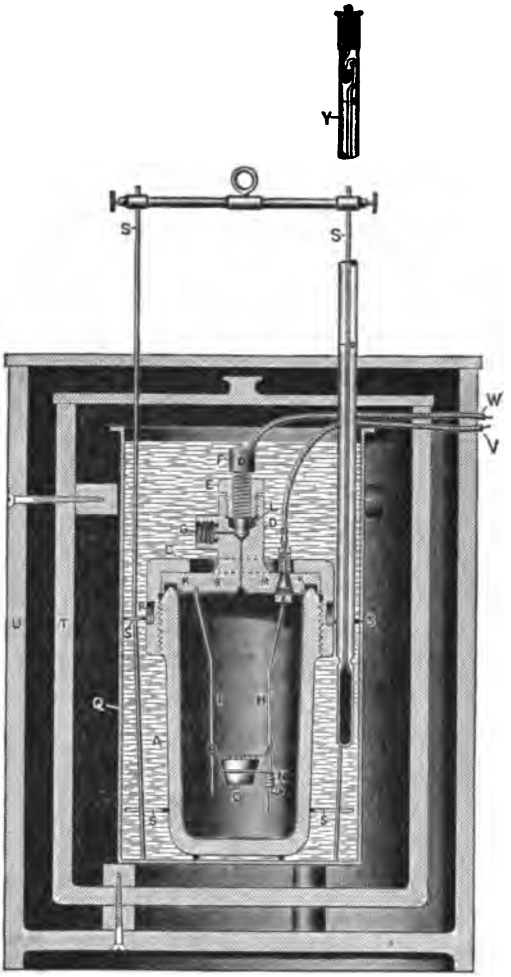

Fig. 14.

Bomb calorimeter ; apparatus as used for actual determination of heats of combustion.

the gauge indicates a pressure of 20 atmospheres. One cylinder full of oxygen will thus suffice for hundreds of combustions.

In the combustions of alcohol two methods were used. In one the alcohol was poured upon cellulose filter blocks. The weighing was made with due precautions to prevent, if possible, any loss by evaporation. The alcohol and filter block were burned in the calorimeter, a correction being applied for the heat of combustion of the filter block used. Eight determinations were made by this method with commercial alcohol of high grade and of different strengths.

The use of filter blocks is objectionable because of the difficulty of avoiding evaporation of alcohol during the process of weighing and transferring to the bomb, and because of the considerable and not absolutely certain correction to be applied for the heat of combustion of the filter blocks used. The first difficulty was avoided entirely and the other partially by the second method employed. In this the alcohol was inclosed in small gelatin capsules by which the evaporation is prevented. The weight of gelatin is also small and the correction for its heat of combustion is less than with the filter blocks. Eight determinations were made with Squibb's alcohol and nine with commercial alcohol, such as was used in the tests in the respiration calorimeter. The results obtained by both methods are shown in Table V.

The heat of combustion when determined by the first method ranged from 7,030 to 7,090, and averaged 7,069 small calories per gram of absolute alcohol.

The results obtained by the second method with Squibb's alcohol ranged from 7,044 to 7,090 small calories, and averaging 7,061 small calories per gram, absolute alcohol. The results of similar determinations with the commercial alcohol ranged from 7,045 to 7,103 small calories, and averaging 7,070 small calories per gram, absolute alcohol. The average of these 17 determinations with alcohol in gelatin capsules gives 7,066 small calories, practically the same result as was obtained by the use of the filter blocks. Considering the range of variation in these different determinations, we would hardly be justified in assuming that the figure 7,067 represents to a very high degree of accuracy the heat of combustion of

TABLE V.

Determination of heats of combustion of alcohol in the bomb calorimeter.

	Ethyl hydroxid by weight.	Heat of combustion per gram actually determined.	Calculated heat of combustion per gram of ethyl hydroxid.
Alcohol contained in filter blocks:	*Per cent.*	*Small cals.*	*Small cals.*
Specimen No. 1	89.9	6,348	7,062
Do	89.9	6,345	7,058
Do	89.9	6,377	7,093
Average of above 3	....	....	7,071
Specimen No. 2	81.3	5,744	7,063
Do	81.3	5,763	7,090
Average of above 2	....	....	7,076
Specimen No. 3	72.5	5,132	7,078
Do	72.5	5,094	7,030
Do	72.5	5,132	7,078
Average of above 3	....	....	7,062
Average of above 8	....	....	7,069
Alcohol contained in gelatin capsules:			
Specimen No. 4	88.0	6,205	7,050
Do	88.0	6,224	7,072
Do	88.0	6,209	7,055
Do	88.0	6,199	7,044
Do	88.0	6,204	7,050
Do	88.0	6,203	7,049
Do	88.0	6,227	7,076
Do	88.0	6,240	7.090
Average of above 8	....	....	7,061
Specimen No. 5	90.61	6,435	7,101
Do	90.61	6,384	7,046
Do	90.61	6,401	7,064
Do	90.61	6,383	7,045
Do	90.61	6,403	7,066
Do	90.61	6,397	7,060
Do	90.61	6,436	7,103
Do	90.61	6,433	7,099
Do	90.61	6,386	7,047
Average of above 9	....	....	7,070
Average of above 17	....	....	7,066
Average of above 25	....	....	7,067

ethyl hydroxid. There is reason to hope, however, that some of the sources of error in the determinations may yet be partially eliminated, and that thus more reliable results may be obtained. It is worthy of note that Berthelot and Matignon[1] obtained the figure 7,068 as the average of two determinations, which gave 7,067.3 and 7,068.5 small calories, respectively, at 13°. The average 7,068 corresponds to 7,079 at 20°, a value very close to those obtained here.

The heat of combustion of alcohol of any given dilution with water is found by multiplying the heat of combustion of 1 gram of absolute alcohol by the percentage present in the specimen. Thus in the specimen of alcohol used in the first three tests the heat of combustion was 7,067 × .9077, or 6,415 small calories per gram.

While the heat of combustion of 1 gram of absolute alcohol is thus 7,067 small calories per gram, this does not represent the amount of heat that is given off by the combustion of 1 gram of alcohol within the chamber of the respiration calorimeter. In the bomb calorimeter all the water vapor formed is condensed within the apparatus, and hence the heat that had been required to vaporize the water is given off again. In the respiration calorimeter, on the other hand, the water passes out as vapor in the ventilating air current. The heat required to vaporize it comes from the combustion of the alcohol, and is not measured by the calorimeter. In order to obtain the total amount of heat given off in the combustion, therefore, the heat actually measured must be added to the amount required to vaporize the excess of water in the outgoing over that in the incoming air current, which is the amount of water vaporized at the expense of the heat produced in the combustion of the alcohol.

3. THE CALORIE HERE USED AS THE UNIT OF MEASURE.

Although the specific heat of water is often taken as unity for all temperatures, it actually varies by an appreciable amount. Hence if the unit of heat be defined as the amount required to raise unit mass of water 1 degree, this unit will be a variable one. The theoretical large calorie, namely, the quantity of heat that will raise a kilogram of water from 0° to 1°, or from 4° to 5°, is a very inconvenient unit in practice. Moreover, the specific heat of water changes quite rapidly at these low temperatures, and hence a higher temperature is more favorable for the working unit. Many authorities take the specific heat of water at 15° C. as unity. Inasmuch, however, as we have used 20° C. as our standard temperature for the respiration calorimeter in experiments with human subjects, and inasmuch also as the specific heat of water has a minimum near to 20° and changes very slowly in that vicinity, we have found it desirable to take as our working unit of heat the calorie at 20°. In other words, the large calorie at 20°, which we designate in the tables as C_{20}, is the amount of heat required to raise the temperature of a kilogram of water 1 degree at 20° C. (that is, from half a degree below 20° to half a degree above). The large calorie at any other temperature is here designated by C_t, and is the amount of heat required to raise the temperature of a kilogram of water 1 degree at the temperature t.

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[1] Ann. Chim. et Phys., 6 ser., 27 (1892), p. 312.

Since the specific heat of water is nearly a minimum at 20° C., our standard calorie, C_{20}, is nearly always less than C_t. This difference is ordinarily neglected. But in the alcohol-test experiments and in the experiment with a human subject to be described later, this difference is, relatively speaking, not inconsiderable. In Table VI. the specific heat of water is given for different temperatures between 0° and 31°. The figures are based upon the experiments of Rowland,[1] Bartoli and Stracciati,[1] Griffiths,[1] and Ludin.[2] The differences in the results obtained by these investigators at temperatures below 22° are very small, and, in view of the care with which their experiments were made, we do not believe that the estimates of this table can be far enough from the truth materially to diminish their value for the present purpose.

It will be seen that at 0° the calorie (C_0) is nearly 1 per cent. greater than C_{20}, and that C_{10} is about three parts in a thousand greater than C_{20}.

The results of all the combustions by the bomb calorimeter, as

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[1] See p. 00 above.

[2] Inaug. Diss., Zurich, 1895; cited by Longuinine, Bestimmung der Verbrennungswärme, Berlin, 1897, p. 17. See discussion of results of experiments on the specific heat of water on pp. 12–20 of this valuable treatise.

TABLE VI.

Specific heat of water at different temperatures referred to that at 20° C. as unity.

Temp. °C.	Specific heat.	Temp. °C.	Specific heat.	Temp. °C.	Specific heat.	Temp. °C.	Specific heat.
0	1.0090	8	1.0039	16	1.0007	24	.9998
1	1.0083	9	1.0034	17	1.0004	25	.9998
2	1.0076	10	1.0029	18	1.0002	26	.9998
3	1.0069	11	1.0024	19	1.0001	27	.9999
4	1.0062	12	1.0020	20	1.0000	28	.9999
5	1.0056	13	1.0016	21	.9999	29	1.0000
6	1.0050	14	1.0013	22	.9998	30	1.0001
7	1.0044	15	1.0010	23	.9998	31	1.0002

well as the measurements by the respiration calorimeter, are to be expressed in terms of C_{20}. To do this it is necessary to know the mean specific heat of water for the range of temperature employed in any given experiment. For example, if water is warmed from 3° to 17° in passing through the absorbed pipes of the respiration calorimeter, the result will be in terms of $C_{(3-17)}$; that is, in terms of the mean calorie from 3° to 17°. This is 1.0032 times as great as the standard calorie C_{20}, whereas C_{10}, the calorie for the mean temperature, is only 1.0029 times as great as C_{20}. In other words, the mean specific heat from 3° to 17° is 1.0032 times the specific heat at 20°, whereas the specific heat at 10°, the mean temperature, is 1.0029 times that at 20°. The difference between the mean specific heat for this range of temperature and the specific heat of the mean temperature is therefore appreciable; this is of course because the variation of the specific heat is not linear. We have accordingly calculated the mean specific heat of the water in every case for the range of temperatures employed, and expressed the heat measured in terms of C_{20}. In the tables this range of temperature is given in the fourth column, and the heat in terms of C_{20} in the fifth column.

4. THE LATENT HEAT OF WATER VAPOR.

The value commonly used for the latent heat of vaporization of water at different temperatures is that given by Regnault[1] as the result of his classical investigations half a century ago. Regnault's formula, which expresses quite closely the results obtained from 63° to 195°, is $H = 606.5 + .305t$. H represents what Regnault called the "total heat" of steam or water vapor, at the temperature t; that is, it is the number of small calories required to raise a gram of water from 0° C. to any temperature t and then completely vaporize it at that temperature. For $t = 100°$, this gives $H = 637$. The "latent heat of vaporization" at any temperature, as distinguished from the "total heat" at the same temperature, is the number of units of heat required to evaporate the water after it has been brought to the temperature of evaporation. Hence at 100° C. the latent heat of vaporization is 537 calories. The formula for the latent heat at any temperature t is

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[1] Mém. Acad. Roy. Sci. Inst. France, 21 (1847), pp. 635–728.

$$L = H - t$$
$$\therefore L = 606.5 + .305\,t - t$$
$$\text{or } L = 606.5 - .695\,t.$$

Regnault's value for the latent heat of steam at 100° is abundantly confirmed by later researches.

At lower temperatures than 63°, however, the method employed was inapplicable because the change of state from water to steam at low pressure is irregular and explosive; hence a different method became necessary. The process was accordingly reversed, and water contained in a small reservoir inside a much smaller calorimeter than that previously used was evaporated at reduced pressure. The heat absorbed in the vaporization of the water (about 5 grams at each experiment) was then determined from the lowering of the temperature of the water which surrounded the vessel in which the evaporation took place. The results, as Regnault himself points out, were subject to comparatively large experimental errors, and were very discordant. The mean results over a range of temperature from 0° to 16° are, however, fairly well represented by the formula given above for higher temperatures.

Starkweather,[1] in a critical review of various determinations of the latent heat of water vapor, quotes the work of Dieterici,[2] Griffiths,[3] and Svensson,[1] as the best that has been done at low pressure and temperatures. Dieterici and Svensson determined with an ice calorimeter the latent heat of water vapor at 0° C. Dieterici's work is very carefully done, and his result is $L = 598.9$ at 0° C., in terms of the quantity of heat required to raise 1 gram of water from 15° to 16° as the unit of heat. Svensson's result agrees very closely with this, being 599.9. Griffiths measured the latent heats of water vapor between 25° and 50° C., and succeeds in representing them by the formula $L = 596.73 - .601t$. Extrapolating to 0° and 100°, this agrees quite well with Dieterici at 0° and Regnault and others at 100°, being 596.73 at 0° and 536.6 at 100°. None of these experiments, however, have been carried out at the temperature commonly used in the respiration calorimeter—*i. e.*, 20° C. Hence we are obliged to deduce the latent heat of vaporization at 20° from formulas given by experiments at other temperatures.

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[1] "Concerning Regnault's calorie and our knowledge of the specific volumes of steam." Amer. Jour. Sci., 7 (1899), p. 13.

[2] Ann. Phys. u. Chem. (Wiedemann), 38, 1889.

[3] Phil. Trans. Roy. Soc. (London), A, 1895.

Using Regnault's original formula, from which $L = 606.5 - .695\,t$ we get $L = 592.6$ at 20° C. Using Starkweather's modification of this formula, which agrees with Regnault's results at temperatures between 63° and 100° better than Regnault's, viz, $L = 598.9 - .558\,t - .00064t^2$, L becomes 587.5 at 20° C. Using Griffith's formula above as quoted $L = 584.5$ at 20° C.

Some preliminary experiments have been made with the respiration calorimeter, with a view of determining this quantity L, *i. e.*, the latent heat of vaporization of water at 20°, under the circumstances of the respiration experiment. The results so far have not, however, given a satisfactory value for this quantity. We have therefore taken 592 as a provisional value for the calculation of the experiments reported in this article, and it would appear that this is probably within 1 per cent. of the truth, although it is possibly more than one per cent. too great. Further investigations at 20° are necessary in order to fix the value more precisely.

5. DESCRIPTION OF THE TEST EXPERIMENTS.

Various substances have been used by experimenters with different forms of respiration apparatus for burning within the chamber in order to deliver known quantities of carbon dioxid and water for the purpose of testing the accuracy of the apparatus and methods.

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[1] Beiblätter, Ann. Phys. u. Chem. (Wiedemann), 20, p. 356.

The chief difficulty is to find substances which can be so burned as to insure complete oxidation of the carbon and hydrogen. The first attempts in this laboratory with the combustion of alcohol were unsatisfactory because the oxidation of the alcohol was incomplete, and a considerable amount of experimenting was necessary in order to learn the conditions under which complete combustion could be secured. As the result a small lamp, such as is ordinarily used with kerosene for illuminating purposes, has been employed. By proper arrangement of wick and chimney the alcohol is burned so completely that no traces of volatilized alcohol, acetone, aldehyde, carbon monoxid, or other substances capable of yielding carbon dioxid upon heating with oxygen could be found among the products of combustion.

A considerable number of alcohol test experiments were made. The individual experiments included from one to seven periods or "runs" of approximately six hours each.

The lamps used in these tests contain approximately 380 grams of alcohol. By adjusting the wick the rate of burning was so regulated as to give off carbon dioxid and water in the desired amounts.

In Table VII. are summarized the principal results of all the alcohol check tests which were made up to the ninth, except the preliminary tests, in which the methods of manipulation were being worked out, and two tests the completion of which was prevented by accident. These individual experiments continued from 5 to 78 hours each, the total time being 317 hours. The rate of burning of the alcohol ranged from 10 to 27 grams per hour, and the strength of the alcohol from 90.26 to 90.77 per cent. Most of the tests were made in alternation with experiments with men, the object being to test the accuracy of the apparatus before and after each of the latter experiments.

It will be noticed that in No. 4 the determination of carbon dioxid was unsatisfactory, though we were unable to decide whether the error was due to imperfect sampling or other cause. The result is not included in the average of Table VII. The measurement of the

TABLE VII.

Summary of nine alcohol test experiments

No.	Date.	Duration.		Alcohol burned.	Carbon dioxid.			Water.			Heat.		
					Required.	Found.	Ratio of amount found to amount required.	Required.	Found.	Ratio of amount found to amount required.	Required.	Found.	Ratio of amount found to amount required.
	1897.	*h.*	*m.*	*Grams.*	*Grams.*	*Grams.*	*Per cent.*	*Grams.*	*Grams.*	*Per cent.*	*Calories.*	*Calories.*	*Per cent.*
1	April 27-29	52	31	955.4	1,657.2	1,657.6	100.0	1,106.1	1,109.7	100.3	6,129.0	6,077.1	99.15
2	May 10-11	29	56	798.8	1,385.6	1,384.4	99.9	924.8	925.0	100.0	5,124.2	5,167.1	100.84
3	May 26-27	33	50	505.4	876.7	887.8	101.3	585.1	627.9	[107.3]	3,242.1	3,217.7	99.25
4	October 27-28	34	33	797.7	1,384.8	1,335.7	[96.5]	925.7	1,007.9	[108.8]	5,120.5	5,141.5	100.41
5	November 2-3	35	09	788.2	1,365.1	1,376.7	100.8	912.3	920.8	100.9	5,048.4	5,050.0	100.03
6	December 2	11	39	245.3	423.1	417.6	98.6	283.7	287.5	101.3	1,564.8	1,556.8	99.48
	1898.												
7	January 6	5	50	112.2	193.5	193.5	100.0	129.8	131.3	101.2	715.7	731.1	102.15
8	January 24-27	77	57	1,607.8	2,784.4	2,769.7	99.5	1,860.8	1,881.6	101.1	10,294.7	10,268.5	99.74
9	May 9	35	55	699.7	1,206.9	1,198.9	99.4	809.3	807.9	99.8	4,463.4	4,466.0	100.05
	Total[1]	317	20		9,892.5	9,886.2	99.9	6,026.8	6,063.8	100.6	41,702.8	41,675.8	99.93

[1] Omitting the carbon dioxid and water in test No. 4 and the water in test No. 3.

heat was, however, very close to the theoretical value. Almost immediately after the close of this test another was made, in which all the results were very closely in accord with the theoretical values. If the determinations of carbon dioxid in test No. 4 are omitted, the maximum variation of the amounts determined from the theoretical amounts given off by the combustion of the alcohol was 1.4 per cent. and the average variation only 0.1 per cent., or 1 part in 1,000 from the theoretical.

In tests Nos. 3 and 4 the determinations of water were unsatisfactory. We are inclined to attribute the errors to variations in the amounts of water condensed upon the absorbers. These results, like that for carbon dioxid in No. 4, are omitted from the averages. Omitting the determinations of water in tests Nos. 3 and 4, the maximum variation, as actually determined from the theoretical amount, was 1.2 per cent. and the average variation only 0.6 per cent. In test No. 7 the proportion of heat measured was larger than usual. It will be observed, however, that this test continued only through one period of six hours. Some time is required to get the apparatus in temperature equilibrium, and the heat measurements of the first experimental period are frequently incorrect on this account. The omission of this experiment would not materially affect the total averages. Omitting this experiment, the maximum variation of the heat actually measured from the theoretical amount was 0.8 per cent. and the average variation only 0.1 per cent.

6. SUMMARY OF TEST EXPERIMENTS.

The accuracy of the methods for the determination of carbon dioxid, water and heat was tested by heat generated in the chamber by passing an electric current through a resistance coil and by burning ethyl alcohol within the chamber. In the electrical tests the measurements of heat generated and found were practically identical, the differences between the theoretical and actual results averaging about 0.1 per cent—that is, about 1 part in 1,000. In the alcohol tests the average amounts found by actual experiment were: For carbon, 99.9 per cent.; hydrogen, 100.6 per cent.; and heat, 99.9 per cent., respectively, of the theoretical amounts.

The determinations of carbon dioxid and water made by burning large quantities of alcohol in the respiration chamber agree reasonably well with each other and with the theoretical amounts. The variations, indeed, are not greater than are found in ordinary laboratory experience when alcohol is burned in the combustion furnace by the usual methods of organic analysis.

The agreement thus shown to exist between the results given by the respiration calorimeter and the bomb calorimeter for the heat of combustion of alcohol, is also very satisfactory when the great difference in the circumstances of the experiments is taken into consideration. In the bomb calorimeter a fraction of a gram of alcohol is absorbed in a small block of cellulose or inclosed in a gelatin capsule and placed in a steel cylinder of perhaps half a liter capacity, which is filled with oxygen at a pressure of 20 atmospheres. An electric current passes through a fine iron wire, melts the latter, and ignites the alcohol, which is completely oxidized in an instant. The increase of temperature of the bomb and the water in which it is immersed gives the quantity of heat evolved, and from that the heat of combustion of a gram of alcohol is computed. In the respiration calorimeter, on the other hand, the alcohol contained in a small lamp burns quietly for many hours in a chamber ten thousand times as large as the bomb. Oxygen is supplied by a continuous current of air pumped through the apparatus, and a considerable portion of the heat of combustion is carried away in the latent heat of the water vapor produced. The sum of the heat measured by the calorimeter and the latent heat of the water vapor collected gives the total heat produced by the burning alcohol. That the average of a series of nine experiments should vary less than 0.1 per cent. from the average of the determinations with the bomb calorimeter seems a gratifying result. Taken in connection with the electrical tests and the determinations of carbon dioxid and water already given, the results show that the respiration calorimeter is an instrument of precision and abundantly capable of doing the work for which it was designed.

IV.

EXPERIMENTS WITH MAN.

The apparatus previously described was designed exclusively for experiments with human subjects. There were several reasons for beginning with men rather than with domestic animals. The study of human nutrition is very important. In the earlier development of the work, when many difficulties were to be overcome, it was very desirable to have inside the apparatus an intelligent person who could make and record important observations during the experiment, rather than an animal whose movements, even, could not be controlled. Indeed, the most advantageous way to develop methods and apparatus for experiments with animals is through such preliminary experience with men. The results of the experience thus far gained are now being utilized both here and elsewhere in planning apparatus and methods to be used not only with small animals, as rabbits, sheep and dogs, but also with larger animals, as horses, oxen and cows.

PLAN OF EXPERIMENTS.

The diet chosen for any given experiment is followed for eight days, of which the last four constitute the period of the experiment proper. In the first or preliminary period, extending through four days, the analyses of feces and urine are made, the data thus sufficing for a digestion and nitrogen metabolism experiment. On the evening of the fourth day the subject enters the respiration chamber, though the actual respiration calorimeter experiment does not begin until 7 o'clock on the morning of the fifth day. The night sojourn in the apparatus suffices to get the temperature of the apparatus and its content of carbonic acid and water into equilibrium, so that accurate measurements may begin with the morning of the fifth day and continue until 7 o'clock on the morning of the ninth day, thus making the duration of this experiment exactly four days. As in the test experiments already reported the determinations of carbon dioxid, water vapor and heat are made in six-hour periods, so that the complete data for an experiment show the total amounts of these compounds given off from the body during the periods ending at 1 p. m., 7 p. m., 1 a. m. and 7 a. m. of each day of the experiment. The urine is also collected and the nitrogen determined for corresponding periods.

The accompanying schedule shows the daily routine followed in the experiment here reported, which was carried out in January, 1898.

TABLE VIII.

Daily program of the experiment.

7.00 a. m.	Rise.	1.15 p. m.	Dinner.
	Pass urine.	4.00 p. m.	Drink 200 grams water.
	Weigh self, stripped.	6.30 p. m.	Supper.
	Weigh absorbers.	7.00 p. m.	Pass urine.
	Collect drip.		Collect drip.
7.45 a. m.	Breakfast.		Weigh absorbers.
10.30 a. m.	Drink 200 grams water.	10.00 p. m.	Drink 200 grams water.
1.00 p. m.	Pass urine.		Weigh self, stripped.
	Collect drip.		Retire.
	Weigh absorbers.	1.00 a. m.	Pass urine.

In this experiment the subject was as quiet as practicable. In the four days of the preliminary period he moved about but little and engaged in no considerable amount of either muscular or mental labor. During the four days passed in the chamber he was likewise quiet. The only muscular work done was that involved in dressing, putting up and taking down the folding chair, table, and bed, weighing himself and the absorbers, taking his meals, and caring for the excreta. He passed a large part of the time in reading and sleeping.

The heats of combustion of the food, feces, and urine were determined in the usual manner by means of the bomb calorimeter.

The determinations of carbon and hydrogen were made in the usual manner. The partially dried samples of food, feces and urine were burned with cupric oxid with the aid of a current of oxygen, and the water absorbed by sulphuric acid, the carbon dioxid by potassium hydroxid.

The percentage composition and heats of combustion of the food used in the experiment here described, and of the feces for the same experiment is shown in Table IX.

TABLE IX.

COMPOSITION OF FOOD MATERIALS, ETC.

Food materials, etc.	Nitrogen.	Carbon.	Hydrogen.	Water.	Protein (N × 6.25).	Fat.	Carbohydrates.	Ash.	Heats of combustion per gram.
Beef, cooked	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]
Butter	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]
Milk, skimmed	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Bread	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Breakfast food, wheat	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Ginger snaps	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	4.358
Breakfast food, maize	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	4.444
Sugar		42.11	6.48				100.0		[illegible]
Feces	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	4.41	[illegible]

EXPLANATION OF THE TABLES.

The results obtained in this experiment are shown in the following tables. Only such of the data are given here as it is believed will be sufficient for a clear understanding of the experiments.

The daily menu was as follows:

TABLE X.

DAILY MENU.

Breakfast.	Grams.	Dinner.	Grams.	Supper.	Grams.
Cooked beef	100	Cooked beef	150	Butter	25
Butter	25	Butter	20	Skimmed milk	390
Skimmed milk	250	Skimmed milk	250	Bread	25
Bread	25	Bread	50	Breakfast food, wheat	75
Breakfast food, maize	50	Breakfast food, wheat	50	Ginger snaps	60
Sugar	25	Sugar	25	Sugar	30
Coffee, about	300	Coffee, about	300	Coffee, about	300

Table XI shows the amount of carbon dioxid eliminated. The table shows the number of liters of air in the ventilating air current and the milligrams per liter of carbon dioxid in the incoming and in the outgoing air during each six-hour period of the experiment. From these data the total excess of carbon dioxid in the outgoing over that from the incoming air is calculated. The analysis of the residual air in the apparatus at the close of each period gives data for the calculation of the increase or decrease of the carbon dioxid in the chamber at the end as compared with the beginning of each period. Applying this correction the actual elimination of carbon dioxid by the subject is obtained for each six-hour period. From this is calculated the total weight of carbon exhaled.

Table XI.

Record of carbon dioxid.

Day.	Period.	Ventilation: number of liters of air.	Carbon dioxid per liter.			Total excess in outgoing air.	Correction for carbon dioxid in apparatus.	Corrected weight carbon dioxid exhaled by subject.	Total weight carbon exhaled in carbon dioxid.
			In incoming air.	In outgoing air.	Excess in outgoing air.				
		Liters.	*Mg.*	*Mgs.*	*Mgs.*	*Grams.*	*Grams.*	*Grams.*	*Grams.*
1	7 a.m. to 1 p.m.	25,712	0.580	9.556	8.976	230.8	+ 3.1	233.9	63.8
	1 p.m. to 7 p.m.	25,987	.563	9.686	9.123	237.1	+14.9	252.0	68.7
	7 p.m. to 1 a.m.	26,785	.622	8.997	8.375	224.3	−14.7	209.6	57.2
	1 a.m. to 7 a m.	26,065	.593	5.891	5.298	138.1	− 2.5	135.6	37.0
	Total......	104,549	.....	....	....	830.3	+ .8	831.1	226.7
2	7 a.m. to 1 p.m.	27,057	.571	8.750	8.179	221.3	+11.3	232.6	63.4
	1 p.m. to 7 p.m.	25,878	.560	9.109	8.549	221.3	+ 5.3	226.6	61.8
	7 p.m. to 1 a.m.	26,652	.612	9.556	8.944	238.4	−12.6	225.8	61.6
	1 a.m. to 7 a.m.	26,011	.629	5.765	5.136	133.6	− 5.2	128.4	35.0
	Total......	105,598	....	.....	....	814.6	− 1.2	813.4	221.8
3	7 a.m. to 1 p.m.	26,342	.624	9.139	8.515	224.3	+19.2	243.5	66.4
	1 p.m. to 7 p.m.	26,492	.721	9.349	8.628	228.6	+ .3	228.9	62.4
	7 p.m. to 1 a.m.	26,147	.722	9.326	8.604	225.0	−10.6	214.4	58.5
	1 a.m. to 7 a.m.	25,163	.727	5.917	5.190	130.6	− 8.1	122.5	33.4
	Total......	104,144	....	.....	......	808.5	+ .8	809.3	220.7
4	7 a.m. to 1 p.m.	26,427	.710	8.973	8.263	218.4	+21.2	239.6	65.3
	1 p.m. to 7 p.m.	25,731	.735	9.525	8.790	226.2	+ .3	226.5	61.8
	7 p.m. to 1 a.m.	26,046	.636	10.021	9.385	244.5	−14.0	230.5	62.9
	1 a.m. to 7 a m.	26,338	.612	5.870	5.258	138.5	− 9.1	129.4	35.3
	Total.....	104,542	.....	......	......	827.6	− 1.6	826.0	225.3
	Total, 4 days...	418,833	......	.....	....		......	3,279.8	894.5
	Av. per day....	104,708	....	......	......	......	.....	819.9	223.6

Table XII. shows the amount of water exhaled. It indicates the number of liters of air in the ventilating air current and the milligrams per liter of water vapor in the incoming air and in the outgoing air after passing the freezers, which latter condense the major portion of the water vapor in the outgoing air current. From these data, together with the amount of water condensed in the freezers, the amount condensed in the chamber as "drip," and the determinations of residual water vapor in the chamber, is computed the total water exhaled by the subject.

TABLE XII.

Record of water.

Day.	Period.	Ventilation; number of liters of air.	Water per liter.			Total excess in outgoing air.	Condensed in freezers.	Condensed in chamber.	Correction for water vapor in chamber.	Total water exhaled.
			In incoming air.	In outgoing air.	Excess in outgoing air.					
		Liters.	*Mg.*	*Mgs.*	*Mg.*	*Grams.*	*Grams.*	*Grams.*	*Grams.*	*Grams.*
1	7 a.m. to 1 p.m.	25,712	0.895	1.301	0.406	10.4	207.5	14.0	−13.5	218.4
	1 p.m. to 7 p.m.	25,987	.816	1.258	.442	11.5	230.2	22.0	+15.3	279.0
	7 p.m. to 1 a.m.	26,785	.792	1.184	.392	10.5	238.1	−18.0	+ 3.1	233.7
	1 a.m. to 7 a.m.	26,065	.731	1.143	.412	10.7	223.0	......	− 6.9	226.8
	Total......	104,549	......	......	......	43.1	898.8	18.0	− 2.0	957.9
2	7 a.m. to 1 p.m.	27,057	.737	1.187	.450	12.2	212.0	3.0	+ .5	227.7
	1 p.m. to 7 p.m.	25,878	.821	1.232	.411	10.6	213.7	− 1.0	+ 2.2	225.5
	7 p.m. to 1 a.m.	26,652	.749	1.221	.472	12.6	230.7	−21.0	+ 4.6	226.9
	1 a.m. to 7 a.m.	26,011	.723	1.219	.496	12.9	208.4	......	− 9.7	211.6
	Total	105,598	....	.....	......	48.3	864.8	−19.0	− 2.4	891.7
3	7 a.m. to 1 p.m.	26,342	.764	1.320	.556	14.6	202.1	13.1	+ 3.8	233.6
	1 p.m. to 7 p.m.	26,492	.772	1.206	.434	11.5	208.3	4.0	+ .5	224.3
	7 p.m. to 1 a.m.	26,147	.740	1.088	.348	9.1	230.6	− 8.0	+10.5	242.2
	1 a.m. to 7 a.m.	25,163	.799	1.271	.472	11.9	209.0	.....	−15.1	205.8
	Total	104,144	...	......	.....	47.1	850.0	9.1	− .3	905.9
4	7 a.m. to 1 p.m.	26,427	.793	1.293	.500	13.2	209.2	41.8	+ 3.9	268.1
	1 p.m. to 7 p.m.	25,731	.726	1.273	.547	14.1	206.2	37.8	+ 2.3	260.4
	7 p.m. to 1 a.m.	26,046	.776	1.128	.352	9.2	235.5	− 6.7	+ 5.4	243.4
	1 a.m. to 7 a.m.	26,338	.715	1.109	.394	10.4	234.6	4.2	−11.6	237.6
	Total	104,542	......	......	.....	46.9	885.5	77.1	......	1,009.5
	Total, 4 days..	418,833	......	......	......	185.4	3,499.1	85.2	− 4.7	3,765.0
	Av. per day..	104,708			.....	......	......	....	....	941.3

The calorimetric results obtained in this experiment are shown in Table XIII. The details of the table and moethds of computation are explained in Bulletin 63 of the Office of Experimental Stations, U. S. Dept. of Agriculture.

TABLE XIII.

Summary of calorimetric measurements

Day	Period.	Heat measured.	Change of temperature of calorimeter.	Capacity correction.	Correction due to temperature of food and dishes.	Corrected heat.	Water vaporized.	Equivalent heat of water vaporized.	Total heat.
		Cals.	°*C.*	*Cals.*	*Cals.*	*Cals.*	*Grams.*	*Cals.*	*Cals.*
1	7 a.m. to 1 p.m.	547.9	+0.12	+ 7.2	− 22.2	532.9	217.9	129.0	661.9
	1 p.m. to 7 p.m.	556.6	− .12	− 7.2	− 37.5	511.9	241.7	143.1	655.0
	7 p.m. to 1 a.m.	472.7	− .05	− 3.0	0	469.7	248.6	147.1	616.8
	1 a.m. to 7 a.m.	277.3	− .03	− 1.8	0	275.5	233.7	138.4	413.9
	Total	1,854.5	....	− 4.8	− 59.7	1,790.0	......	557.6	2,347.6
2	7 a.m. to 1 p.m.	490.7	+ .04	+ 2.4	− 22.9	470.2	224.2	132.7	602.9
	1 p.m. to 7 p.m.	489.2	+ .08	+ 4.8	− 28.3	465.7	224.5	132.9	598.6
	7 p.m. to 1 a.m.	497.4	− .15	− 9.0	0	488.4	243.3	144.1	632.5
	1 a.m. to 7 a.m.	289.2	+ .15	+ 9.0	0	298.2	221.3	131.0	429.2
	Total......	1,766.5	....	+ 7.2	− 51.2	1,722.5	......	540.7	2,263.2
3	7 a.m. to 1 p.m.	524.6	+ .08	+ 4.8	− 21.2	508.2	216.7	128.3	636.5
	1 p.m. to 7 p.m.	512.1	+ .08	+ 4.8	− 31.8	485.1	219.8	130.1	615.2
	7 p.m. to 1 a.m.	486.6	− .18	−10.8	0	475.8	239.7	141.9	617.7
	1 a.m. to 7 a.m.	296.1	+ .10	+ 6.0	0	302.1	220.9	130.8	432.9
	Total......	1,819.4	....	+ 4.8	− 53.0	1,771.2	.....	531.1	2,302.3
4	7 a.m. to 1 p.m.	503.3	− .05	− 3.0	− 21.3	479.0	222.4	131.7	610.7
	1 p.m. to 7 p.m.	536.4	+ .05	+ 3.0	− 29.2	510.2	220.3	130.4	640.6
	7 p.m. to 1 a.m.	497.5	− .10	− 6.0	0	491.5	244.7	144.9	636.4
	1 a.m. to 7 am.	290.7	+ .04	+ 2.4	0	293.1	245.0	145.0	438.1
	Total......	1,827.9	....	− 3.6	− 50.5	1,773.8	......	552.0	2,325.8
	Total, four days	7,268.3	....	+ 3.6	−214.4	7,057.5	......	2,181.4	9,238.9
	Av. per day	1,817.1	....	+ .9	− 53.6	1,764.4	....	545.3	2,309.7

As has been previously mentioned, the urine was collected in six-hour periods beginning at 7 a. m. (the hour of beginning and ending the experiment). The amount of nitrogen in the urine from 7 a. m. of one day to 7 a. m. next day is taken as a measure of the protein metabolized during this period. Of course this makes no allowance for the nitrogen lag, *i. e.*, the period between the metabolism of the nitrogen and its excretion, but it was considered that the error thus introduced might be ignored, since the subject had been living on the same diet and had had the same exercise from day to day for the experimental period of four days and the previous four days of the preliminary experiment also. The amount of nitrogen in the urine for 12 to 24 hours after the close of the experiment was also determined in six-hour periods, although these values are not recorded here. In Table XIV. is summarized the amount of urine for each day of the experiment, together with the percentage and amounts of nitrogen and carbon, and the heat of combustion per gram and the total heat of combustion of the urine.

TABLE XIV.

Amount and composition of urine.

	Amount.	Nitrogen.		Carbon.		Heat of combustion.	
						Per gm.	Total
	Grams.	*Per cent.*	*Grams.*	*Per cent.*	*Grams.*	*Calories.*	*Calories.*
First day.	1,855.3	1.01	18.75	0.69	12.80	0.082	152
Second day.	1,977.6	.95	18.75	.65	12.79	.081	160
Third day.	1,510.6	1.21	18.28	.83	12.50	.095	143
Fourth day.	1,358.9	1.32	17.89	.90	12.19	.102	139
Total, four days.	6,702.4	....	73.68	....	50.28	....	594
Average per day.	1,675.6	....	18.42	....	12.57	....	149

The data previously given serve for the calculation of Tables XV. and XVI., which show the gain or loss of nitrogen, carbon, protein and fat and the comparison of the estimated heat of the material oxidized in the body with the heat actually measured in this experiment. The calculations are partially explained by the letters and algebraic formulas at the tops of the columns. Thus column *d* indicates the gain or loss of nitrogen, and is computed by adding the amounts in the feces and urine, as shown in columns *b* and *c*, and subtracting this sum from the amount in the food as shown in column *a*. Protein contains, approximately, 16 per cent. of nitrogen, and hence by multiplying the nitrogen loss by 6.25 we get the quantity of protein of the body which must have been oxidized; this is given in column *e*.

The numbers in columns *f*, *g* and *h* are the results of direct chemical analysis of samples of food, feces and urine, the details of which are not here reported. The numbers in column *i* are taken from Table XI., and column *j* gives the net gain of carbon during each day of the experiment.

As protein contains, approximately, 53 per cent. of carbon, the amount of carbon yielded by the daily consumption of body protein is computed by multiplying the numbers of column *e* by .53; these results are given in column *k*. But there was a net gain to the body of carbon, which is explained by the inference that there was more carbon stored in fat accumulated than was lost in protein

TABLE XV.

Income and outgo of nitrogen and carbon, with estimated gain or loss of protein and fat.

	Nitrogen.				*e.* Protein loss $=d\times 6.25$.	Carbon.					*k.* Carbon in protein lost $=e\times 0.53$	*l.* Carbon in fat gained $=j-k$.	*m.* Fat gain $=l+0.765$.
	a. In food.	*b.* In feces.	*c.* In urine.	*d.* Net loss $=a-(b+c)$.		*f.* In food.	*g.* In feces.	*h.* In urine.	*i.* In respiratory products.	*j.* Net gain $=f-(g+h+i)$.			
	Gms.	*Gms.*	*Gms.*	*Gms.*	*Gms.*	*Gms.*	*Gms.*	*Gms.*	*Gms.*	*Gms.*	*Gms.*	*Gms.*	*Gms.*
First day.	19.1	1.2	18.7	−0.8	− 5.0	261.5	13.3	12.8	226.7	+ 8.7	−2.7	+11.4	+14.9
Second day.	19.1	1.3	18.8	−1.0	− 6.3	261.6	13.4	12.8	221.8	+13.6	−3.3	+16.9	+22.1
Third day.	19.1	1.2	18.3	− .4	− 2.5	261.5	13.3	12.5	220.7	+15.0	−1.3	+16.3	+21.3
Fourth day.	19.1	1.3	17.9	− .1	− 0.6	261.6	13.4	12.2	225.3	+10.7	− .3	+11.0	+14.4
Total.	76.4	5.0	73.7	−2.3	−14.4	1,046.2	53.4	50.3	894.5	+48.0	−7.6	+55.6	+72.7
Average per day.	19.1	1.2	18.4	− .6	− 3.6	261.5	13.3	12.6	223.6	+12.0	−1.9	+13.9	18.2

oxidized. We get the quantity of carbon stored in body fat by adding the numbers of columns *j* and *k*, and the sums are shown in column *l*. Finally, since fat contains about 76.5 % of carbon, we obtain the quantities of fat daily stored in the body by dividing the numbers of column *l* by .765, and these quantities are shown in column *m*.

In Table XVI. the values given in columns *n*, *o* and *p* are actual heats of combustion as determined by the bomb calorimeter. The values in columns *q* and *r* are obtained by the use of the factors 5.5 and 9.4 representing the average heat of combustion of 1 gram of protein and fat, respectively. The values in column *t* are obtained from Table XIII.

TABLE XVI.

Income and outgo of energy.

	Energy (heats of combustion).						*t.* Heat actually measured.	Ratio of heat measured to that of material actually oxidized. $t \div s$.
	Actually determined.			Estimated.				
	n. Of food.	*o.* Of feces.	*p.* Of urine	*q.* Of protein lost $=e \times 5.5$	*r.* Of fat gained $=m \times 9.4$	*s.* Of material actually oxidized in body $=n-(o+p+q+r)$.		
	Calories.	*Calories.*	*Calories.*	*Calories.*	*Calories.*	*Calories.*	*Calories.*	*Per Cent.*
First day.	2,717	142	152	−27	+140	2,310	2,348	
Second day.	2,717	142	160	−35	+208	2,242	2,263	
Third day.	2,717	142	143	−14	+200	2,246	2,302	
Fourth day.	2,717	142	139	− 3	+135	2,304	2,326	
Total.	10,868	568	594	−79	+683	9,102	9,239	......
Av. per day.	2,717	142	149	−20	+171	2,275	2,310	101.5

RESULTS OF THE EXPERIMENT WITH A HUMAN SUBJECT.

Table XV. shows that the subject under experiment eliminated daily on an average 19.7g. of nitrogen while he received only 19.1 g. of nitrogen in his food. This daily loss of nitrogen is taken to indicate a daily metabolism of protein, the heat of combustion of which is given in column *q* of table XVI. The energy of the material actually oxidized in the body, as given in column *s*, is obtained as follows : Taking the first day as an example, we subtract the heat equivalent of feces and urine, 294 calories, from the total heat value of the food, 2,717. This leaves 2,423 calories. But 140 was stored in the body as fat while 27 was realized by combustion of body protein. Hence 113 calories was stored over and above that derived from body protein, and subtracting 113 from 2,423 we have 2,310 calories as the net amount of energy that should have appeared as heat when all mechanical work of the body, internal and external, has been converted into heat. The average for the four days was 2,275, while the average quantity of heat measured by the calorimeter, including the latent heat of water vapor carried away in the air current, as shown in Table XIII., was 2,310 calories. This is a difference of 35 calories, or 1.5 %.

In later experiments, about to be reported, the differences between income and outgo of energy average somewhat nearer to the theoretical; in other words, in these more accurate experiments not far from 99 per cent. of the potential energy of the material metabolized and oxidized in the body is accounted for in the kinetic energy given off in the forms of heat and external muscular work.

In how far this fairly close agreement is due to a counterbalancing of errors it is impossible to say. But in view of the physiological difficulties in the way of absolutely accurate results, and the evident possibility of minor errors in the purely chemical and physical determinations, and likewise in the factors used for computation, this agreement seems to us very satisfactory. We believe that it marks a stage in the development of the apparatus and methods sufficient to warrant extended series of experiments upon various questions connected with the laws of nutrition, and such experiments have been begun. Efforts are being made at the same time to eliminate part at least of the experimental errors. At present these efforts are chiefly in the direction of improvement of methods of sampling and analyzing the food materials and excretory products, and finding of minor sources of error in the determination of carbon, hydrogen and heat given off in the respiration chamber, and the direct determination of the oxygen of income and outgo. Minor alterations are also being made in the apparatus and the methods of its manipulation by which it is hoped that somewhat greater accuracy may be secured.

Mechanical Efficiency of a Man.

In May, 1897, an experiment was carried out in which the subject under investigation performed a considerable amount of muscular work. This was converted into heat by means of a small dynamo belted to a stationary bicycle. The current generated in the dynamo passed through an incandescent lamp, and all the heat produced, either by friction or by the electric current, was measured by the calorimeter along with that given off by the body directly. This experiment was less accurate than the one reported above for two principal reasons: first, because the methods of sampling the food were then less accurate than in the later experiments, and second, because the large amount of heat evolved during working hours, and the sudden changes of rate from work to rest, made it impossible to measure the heat as accurately as in the "rest" experiment. The average number of calories measured daily was 3,726, the number which corresponds to the total material oxidized was 3,830; thus the difference was 2.7 per cent. of the latter amount. The amount of work done by the subject on the bicycle-dynamo was separately determined. This was done by adding to the energy of the current, as measured by a voltmeter and an ammeter, the energy required to overcome the friction of the bicycle-dynamo. The latter was determined by driving the apparatus by the dynamo used as a motor, and measuring the energy required.

The average work done was the equivalent of 256 large calories per day. This is about 40 watts for nearly eight hours, or 109,000 kilogrammeters, or 788,000 foot-pounds, or 394 foot-tons. The total energy actually yielded by the body, including that derived from the food and the body fat oxidized was 3,726 large calories daily. Dividing the work done by the total energy yielded by the body we have .07 or 7 per cent. as the mechanical efficiency.

But inasmuch as the energy received by the body is largely expended in the internal physiological work and in maintaining the temperature of the body, we should charge against the work done only the excess of energy absorbed in the work experiment over that required in a rest experiment. The average amount of energy used in several rest experiments with the same man has been about 2,500 calories. This subtracted from 3,726 leaves 1,226 as the excess of the work experiment. 256 ÷ 1,226 = .21 Hence we have 21 per cent. as the efficiency of conversion of potential energy of food into mechanical energy.

The smaller value of the efficiency, namely 7 per cent. is greater than that of small steam engines, while the larger value, 21 per cent. equals or exceeds that of the best compound condensing engines with the highest efficiency boilers. A large part of the excess of heat in the work experiment is carried away in the latent heat of water vapor of perspiration. This may be likened to the latent heat of the expanded steam escaping from the cylinder of a steam engine. It is probable that other experiments may give a still larger value for the efficiency of man as a machine for converting the potential energy of food into mechanical energy. For man in this experiment was unused to the bicycle, and the work done by him was less than his maximum capacity, and was certainly very much less than the maximum capacity of an experienced bicyclist. As to the obscure but very interesting question of how this conversion is effected, whether it is a conversion of chemical potential energy directly into mechanical energy, or whether the material of food or muscle upon oxidation first yields heat which is subsequently converted into work by a process equivalent to that of a heat engine, or whether it is some other process, these experiments give no information.

GENERAL SUMMARY.

The attempt has been made in the preceding pages to describe (1) a new form of respiration calorimeter and the methods of its use, and (2) several experiments in which the apparatus and methods have been employed.

The experiments here described had a two-fold purpose: To test the accuracy of the apparatus and methods, and to determine the balance of income and outgo of matter and energy in the body. They are preliminary to more extended research upon some of the fundamental problems of nutrition.

The name here used for the apparatus, "respiration calorimeter," is suggested by the fact that it is essentially a respiration apparatus with appliances for calorimetric measurements. As a respiration apparatus it is similar in principle to that of Pettenkofer.

The accuracy of the apparatus and of the methods for the determinations of carbon dioxid, water and heat was tested by heat generated in the chamber by passing an electric current through a resistance coil and also by burning ethyl alcohol within the chamber. In the electrical tests the measurements of heat generated and found were practically identical. In the alcohol tests the average amounts found by actual experiment were: For carbon, 99.9 per cent.; hydrogen, 100.6 per cent.; and heat, 99.9 per cent. of the theoretical amounts. It thus appears that this apparatus when used for the analysis of alcohol and the determination of its heat of combustion gives results nearly, if not quite, as accurate as are obtained by the ordinary laboratory methods which can be used only with small amounts.

The measurements of heat given off from the body of a man inside the chamber are so delicate that very slight bodily movements, such as rising from a chair or turning over in bed, are immediately noticed by the observer, who is constantly watching the galvanometer and thermometers.

The experiments with men, one of the earlier of which is here reported, were undertaken for the study of several problems. The question especially considered in this article is this: Is the energy given off from the body in the form of heat, or of heat and external muscular work, equal to the potential energy or heat of combustion of the material actually burned in the body? In other words, when the compounds of the food and the body—proteids, fats, and carbohydrates—are burned is their potential energy transformed into the equivalent kinetic energy and into forms which can be measured by the means here used? Or, to state the question more broadly, does the law of the conservation of energy obtain in the living organism?

In the experiment reported above the outgo apparently exceeds the income by about 35 calories per day. This quantity is really quite small. It would correspond to the potential energy of about 4 grams of body fat, or nearly the same weight of butter, or 9 grams of sugar, or 14 grams of bread.

The sources of error and uncertainty in these experiments can, it seems to us, be divided into two classes: those incident to the experimental work as such, and those due to uncertainty as to the physical, chemical and physiological constants used as factors in the computations.

So far as the sources of error are those of chemical and physical analyses and manipulation, the attempt is being made to reduce them to a minimum. In so far as they are of physiological origin and practically beyond control the attempt is made to eliminate them so far as practicable by long experimental periods and by duplication of the experiments.

Meanwhile it is safe to say that in view of the physiological uncertainties and sources of error and the probable incorrectness of some of the physical and chemical constants employed, the differences between the estimated income and measured outgo of energy in the experiments with men are not at all surprising.

In view of these defects and sources of error in methods and apparatus we would perhaps be unwarranted in assuming that the experiments thus far made completely demonstrate the application of the law of the conservation of energy in the human organism. They do, however, seem to us to be reasonably near to such demonstration.

It is certainly safe to assume that the principle followed in the experiments is correct, and that the apparatus and methods are accurate to the degree required for the experimental study of a large variety of the fundamental problems of biological chemistry and physics. Among these are the metabolism of energy and the production of heat by the body in the performance of its ordinary functions, as circulation, respiration and digestion; the relations of muscular and mental work to the metabolism of matter and energy; the demands of the body for nutriment under different conditions of work and rest, and under different conditions of health and disease; the duties performed by the different nutrients of food in supplying the needs of the body; and finally, the nutritive values of food materials and the amount and proportions best adapted to the needs of people of different classes, with different occupations, and in different conditions of life.

Wesleyan University, Middletown, Conn., July, 1899.

NEW BOOKS.

Radiation, an Elementary Treatise on Electro-magnetic Radiation and on Röntgen and Cathode Rays. By H. H. Francis Hyndman, B.Sc. Pp. 370. New York, The Macmillan Co.

The more or less arbitrary division of the subject of physics into different branches has usually resulted in leaving in the minds of elementary students a rather indefinite idea of the borderlands of these branches. In this book the reader will find much help in a subject which extends into the subjects of sound, heat, light and electricity. As Professor S. P. Thompson, in the preface to this book, says, "It is just these border regions which, neglected in the text-books, are the richest fields for research." Most of the work which has been done in these fields is inaccessible to many readers, so that such books as this one are not only necessary but are exceedingly wholesome.

The subject is divided into three parts. The first treats of the wave motions in matter or sound. The second, which occupies about half of the book, is devoted to electro-magnetic radiation or wave motions in the ether, ranging from the long electrical waves through the whole gamut to the short ultra-violet waves. In the last part of the book is given a clear and fairly complete treatment of those radiations which have not as yet been definitely classified as Cathode, Röntgen and Becquerel or Uranium rays. The book may be looked upon as a summary in a more or less popular manner of the work which has been done in recent years in these different kinds of radiation. The current theories in each case are discussed and, in general, are well handled. While the properties of the ether have been discussed rather fully, some parts of it have not received as clear a treatment as could be wished. This is especially true of Maxwell's theory. The matter condensed into pages 46 and 47 if expanded only a little more, would have increased the utility of the book.

It is to be regretted that the author found it necessary to introduce several new terms. As in general the introduction of new words into our scientific vocabulary is to be deprecated, there should exist a great demand to warrant the introduction of even one. The author has used *radiable* or *transradiable* instead of the word "transparent," and *nonradiable* instead of "opaque," the excuse being that the word "transparent," on account of its ordinary use, becomes ambiguous when used, for example, with reference to Röntgen rays. It is not clear how the mere change of a word would clear up the ambiguity. As evidence that the author himself has not avoided it, one has only to read the paragraph about the middle of page 263. In using the words "transparent" and "opaque" it is only necessary to specify when using the words what kind of radiation one is referring to, as "transparent to Röntgen rays," or "opaque to ultra-violet rays," and this is necessary when using the new terms. The author has called electrical waves *infralacunal*, and the shorter waves, commonly called "light" waves, *ultralacunal*. As long as the gap or *lacuna* between the electrical waves and light waves exists there seems to be no urgent need for a change. When the gap is filled, if ever, then there may be a time for distinctions to be more clearly made and in that case the names suggested by the author will not suffice.

The great amount of work necessary in compiling a summary of such a character as this is seldom appreciated, and the reader of such a book should be prepared to overlook some errors. In this case there are not a great many, and these are not serious. One of the most misleading is the reference on pages 277 and 280 to the work done by Sagnac. From the author's statement it seems that Sagnac confirmed Becquerel's experiments on the reflection, refraction and polarization of Becquerel's rays. The article by Sagnac was not an account of his own experiments, but merely a summary of the work of Becquerel. So far as I have been able to learn no one has confirmed Becquerel's work on the reflection, refraction and polarization of these rays. The point is worth calling attention to as from more recent work (Rutherford, Phil. Mag., 47, p. 109, 1899, also Becquerel, Comptes Rendus, 128, p. 771, March, 1899), there exists doubt as to whether Becquerel rays possess these properties or not. This error may be due to the fact, that on account of the extent of field covered, the author depended on abstracts rather than on the original papers themselves. In the table on page 49, which is intended to show the entire range of ether vibrations, the author has omitted the work of Schumann (Abstracted in Beiblätter, p. 187, 1894), who measured ultra-violet wavelengths much shorter than any given in the table.

The work is well worth the perusal by students or teachers, or by any one desirous of getting better or clearer ideas of the various phenomena met in this very interesting field. As throughout the book references to the original sources have been given, it will serve as a useful bibliography to more advanced students.

O. M. Stewart.

NOTES.

American Association for the Advancement of Science.—The forty-eighth meeting of the Association was held at Columbus, Ohio, August 19–26, under the presidency of Dr. Edward Orton. The buildings of the Ohio State University afforded ample and unusually comfortable accommodation for the sectional meetings and the General Sessions. All things conduced to the uninterrupted work of the section, the excursions and entertainments being so arranged as not to interfere therewith, and the meetings of the council and executive committees being of moderate duration. That the General Session has outlived its years of usefulness was very evident; valuable time being spent in secretaries' announcements, which would be given sufficient publicity if inserted in the daily programme and posted on the bulletin board. The elimination or curtailment of these sessions is much desired.

Before Section B, in addition to the address by Vice-President Elihu Thomson, on "The Field of Experimental Research," the following forty-two papers were presented:

Apparatus for the demonstration of the varying currents in the different conductors of a rotary converter, F. C. Caldwell; A New Graphical Method of Constructing the Entropy-temperature Diagram from the Indicator Card of a Gas or Gil Engine, H. T. Eddy; Compound Harmonic Vibration of a String, W. Hallock; A New Form of Electrical Condenser having a Capacity Capable of Continuous Adjustment, L. J. Briggs; Time of Perception as a Measure of the Intensity of Light, J. McK. Cattell; Relations of Time and Space in Vision, J. McK. Cattell; The Musical Scales of the Arabs, C. K. Wead; Medieval Organ Pipes and their Bearing on the History of the Scale, C. K. Wead; Electrical Anesthesia, E. W. Scripture; An Absolute Determination of the E. M. F. of a Clark Cell, H. S. Carhart and K. E. Guthe; Quantitative Investigation of the Coherer, A. Trowbridge; Polarization and Polarization-Capacity, K. E. Guthe and M. D. Atkins; Current and Voltage Curves in the Magnetically Blown Arc and in the Aluminum Electrolytic Cell, R. A. Fessenden; Some New Apparatus—Tachometer, Chronograph, Data Collector, Induction Coil, Balance for Standardizing Ampèremeters, Standard of Induction, R. A. Fessenden; Measurement of Magnetism in Iron and the Relation between Permeability and Hysteresis, R. A. Fessenden; Polarization and Internal Resistance of the Copper Voltameter, B. E. Moore; Concerning the Fall of Potential at the Anode in a Geissler Tube, C. A.

Skinner; The Equipment and Facilities of the Office of U. S. Standard Weights and Measures for the Verification of Electrical Standard and Measuring Apparatus, F. A. Wolff, Jr.; An Experimental Test of the Accuracy of Ohm's Law, F. A. Wolff, Jr.; March Weather in the United States, etc., O. L. Fassig; A new Spectrophotometer and a Method of Optically Calibrating the Slit, D. B. Brace; On Achromatic Polarization in Crystalline Combinations, D. B. Brace; On the Nature of Electricity and Magnetism and a Determination of the Density and Elasticity of the Ether, R. A. Fessenden; Advances in Theoretical Meteorology, C. Abbe; on Differential Dispersion in Double Refracting Media, E. J. Rendtorff; Location of Smokeless Powder Discharge by Means of Colored Screens, R. A. Fessenden; a Method for the Study of Phosphorescent Sulphides, F. E. Kester; Accidental Double Refraction in Colloids and Crystalloids, B. V. Hill; Note on the Age of the Earth, R. A. Fessenden; a Bolometric Study of the Radiation of a Black Body Between 600° and 1100° C., C. E. Mendenhall; a Bolometric Study of the Radiation of an Absolute Black Body, F. A. Saunders; on Thermodynamic Surfaces of P. V. T. for Solid, Liquid and Gaseous State, F. E. Nipher; On the Escape of Gases from the Planets According to the Kinetic Theory, R. S. Cook; Relation of Magnetization to the Modulus of Elasticity, J. S. Stevens; On Flutings in Kundt's Tube, S. R. Cook; the Dielectric Strength of Oils, Thos. Gray; Some Unexpected Errors in Wattmeter Measurements, Thos. Gray; Note on the Preparation of Reticles, D. P. Todd; the Nature of Spoken Vowels, with Reference to the Theories of Helmholtz and Hermann, E. W. Scripture; Pressure and Wave-length, J. F. Mohler; the Attenuation of Sound and the Constant of Radiation, of Air, A. Wilmer Duff; Optical Calibration of the Slit of a Spectrometer, E. V. Capps.

The Association next meets at Columbia University, New York city, Monday, June 25th, under the presidency of R. S. Woodward. The officers of Section B elected for the New York meeting are: Vice-President, Ernest Merritt, Ithaca, N. Y.; Secretary, R. A. Fessenden, Allegheny, Pa.

An invitation was received to participate in the International Congress of Physicists to be held in Paris; a committee from Section B was appointed to acknowledge the invitation and to see that the Association is represented at the Congress.

International Congress of Physicists.—The French Physical Society has taken the initiative in arranging for an International Congress of Physicists to be held in connection with the Paris Exposition, from the 6th to the 12th of August, 1900. American physicists are invited to attend the Congress and to participate in its proceedings. Those who desire to be present are requested to communicate with the secretaries: Ch. Ed. Guillaume, au Pavilon de Breteuil, Sèvres (Seine-et-Oise), and Lucien Poincaré, 105 bis, Boulevard Raspail, Paris.

ERRATUM.

Vol. IX., No. 3, Plate I. (Ramsey), opposite page 190.

For No. 2 read No. 33.
" " 4 " " 32.
" " 15 " " 11.
" " 11 " " 15.
" " 32 " " 4.
" " 33 " " 2.

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Volume IX. *November–December, 1899.* *Number 5.*

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THE

PHYSICAL REVIEW.

ON THE FREEZING POINTS OF AQUEOUS SOLUTIONS OF NON-ELECTROLYTES.

By E. H. Loomis.

THE object of the work which is presented in this paper has been to test the validity of the van't Hoff constant for dilute aqueous solutions of non-electrolytes. Much work has already been done in this region, but it has been mostly confined to the investigation of cane sugar, ethyl alcohol and urea. The results in general have not been in accord with the van't Hoff theory. While the great confusion which the earlier results presented has largely disappeared with the more recent advances in the methods of measurement, still there remains much disagreement in the latest observations of the most careful observers, even in cases where the apparent exactness of the methods had lead the observers to regard their results as final.

Thus Abegg[1] finds for the molecular depression in the case of

Cane Sugar,	1.86
Ethyl Alcohol,	1.79
Urea,	1.87
Dextrose,	1.81

While Raoult's[2] latest values give for

Cane Sugar,	1.87
Ethyl Alcohol,	1.83

These results can not be looked upon as *constant*, since the estimated experimental errors are of a much smaller order than would be required for such a conclusion.

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[1] Zeit. Phys. Chem., XX., 1896, p. 207.

[2] Zeit. Phys. Chem., XXVII., 1898, p. 617.

THE METHOD.

The apparatus is the same as that originally used by the writer, and described in the PHYSICAL REVIEW, Vol. I., 1893. The method is likewise unchanged in all essentials. It will be remembered that this method enabled the writer to overcome the most serious difficulty met with in attempts to measure the freezing points of solutions, namely, that of bringing the solution and its "ice" to their actual temperature of equilibrium. This difficulty is so troublesome and the experimental errors which arise in this connection are so great that I am not surprised that the complete solution of this difficulty after many months of the most tedious work left me entirely unarmed against another source of error much less difficult to overcome and of the most self-evident nature. This difficulty concerns itself with the manipulation of the *thermometer*. It was found necessary at the very outset to keep the thermometer at 0° C. when not in actual use. To effect this the thermometer was packed, *its whole length*, in ice in the ice cellar of the Strasburg Laboratory. Thus the *stem*, as well as bulb, was at 0° C. The temperature of the room in which the observations were made was on the average about 5° C., and it is thus obvious that the *stem* of the thermometer was gradually warming during the first series of observations each working day.

Hence these observations were lower than they would have been if the stem had come at once to the temperature of the room. These first observations were uniformly the ones which determined the zero point of the thermometer for the day, and thus all such zero points may be said to have been "too low." Since the stem range was 2°.5 C. and the thermometer was of the enclosed type with a thick glass wall, it is easy to believe that the error arising from this very obvious source could have reached as much as 0°.001 C. To avoid this it is, of course, only necessary to place the lower portion of the thermometer in ice and water and leave the "stem" at room temperature. This change was introduced at the beginning of the 1896 series. A second source of error arises also from the manipulation of the thermometer. While it is of much less importance than the one already described, its discovery has led to a real simplification of the practice of the method, and so deserves attention. It relates also to the stem of the thermometer. That portion of the stem below the cork which supports the thermometer in the freezing vessel and above the surface of the solution must, of course, have a constant temperature if a series of independent observations on the freezing point of a given liquid are to agree. This was accomplished in the earlier work by keeping the freezing vessel with its contents for 20 minutes in a bath of ice and water before each observation of a freezing point. It was then transferred to a freezing mixture to induce freezing of the solution and then to the "protection bath" where the solution and its "ice" were allowed to come to equilibrium. While the great uniformity with which these changes were made would preclude the possibility of any very serious lack of constancy in the temperature of this intermediate portion of the stem, still there is a source of danger here. This becomes the more evident when it was discovered that, although the *length* of this unmarked portion of the stem is only that of 4 degrees on the thermometer, *actual measurement of the temperature range showed it to be 10 degrees.* This fact was unknown to me until after all the experimental work had been finished. It now seems probable that the small remaining experimental error of the present method will be eliminated when the difficulty is removed. Obviously it is necessary to have a thermometer made with the capillary reduced to the smallest possible size in this portion.

Without knowing at the time the real nature of the source of error I was always aware of its existence and overcame it by the long exposure of the whole apparatus to a temperature of 0° C. To still further overcome this difficulty and to facilitate the practice of the method the following change of procedure has been made. The freezing vessel, holding the thermometer and solution, is placed in a freezing mixture until an overcooling of 0°.3 C. is produced. It is then placed directly in the "protection bath," whose temperature is also 0°.3 C. below that of the freezing point to be observed. Here it is allowed to stand 15 minutes. It is then "touched off" with a tiny crystal of ice. The stirring and jarring are then begun and continued for two minutes as usual. A reading of the thermometer is then made.

The Accuracy of the Method.

It is plain that a method which claims to have any high degree of exactness must fulfil two experimental conditions:

1. In a series of observations on a given freezing point, the various observations must have a narrow range of variation. The present method originally showed a range of about 0°.0008 C. in a series of five entirely independent observations. By lowering the room temperature to about 0° C. this variation was reduced to the average value, 0.°0005 C., in the 1896 series, while the slight change in the practice of the method which allows the freezing vessel to stand in the "protection bath" instead of the ice and water bath for the fifteen minutes preceding the observation has reduced this to 0.°00046 C. in the present series. Many series of observations on the zero point of the thermometer gave the same result for each individual observation, while in some cases of the more concentrated solutions the variation has amounted to as much as 0.°0015 C. It is to be noticed that the variation, which now remains, may be wholly accounted for by a lack of uniformity in the temperature of the middle portion of the stem of only 0.°25 C. I am of the opinion that when this source of error is wholly suppressed, this very slight lack of constancy in a series of successive observations will be reduced still further.

2. The observed zero points determined from day to day on the thermometer must show only small variation. An examination of such a series of zero points throws much light upon the exactness of the method, since it is evident that any lack of absolute constancy in such a series must be accounted for either by assuming experimental errors, or irregular changes in the thermometer itself. When the variation is large the method is proportionally inexact. Unfortunately these zero points are generally not published. The latest work of Abegg is, however, an exception, and this example will serve to illustrate the importance of this criterion of a method's accuracy. He publishes a series of eight such zero points, presumably in the order in which they were made, though he fails to give

the dates.[1] I allow myself the privilege of submitting the series together with the barometer at the time.

Zero Point.	Barometer.
0°.7741	747 mm.
.7738	744.5
.7736	741
.7733	742
.7724	741
.7681	736.5
.7670	732.5
.7668	727.5

The total variation is 0.°0073 C. Reducing the readings to a common air pressure, say 740 mm. by use of a correction factor which the author himself experimentally determined (0°.0002 C. per 1 mm.). The series becomes

0.7727,	0.7729,	0.7734,	0.7729,
0.7722,	0.7688,	0.7685,	0.7693.

The range is thus reduced to about 0°.005. Thus of the original variation of some 0°.007 C. in *eight* observations, only 0.002 are accounted for in an experimental way, and yet he says of this series:

"Die Variationen der Einstellung rühren jedoch, wie eine nährere Betrachtung der Zahlen zeigt, sicher nicht allein von den Luftdruckänderungen her, sondern dürften auch kleinen zufälligen Verunreinigungen des destillirten Wassers zuzuschreiben sein, auf dessen genau gleiche Beschaffenheit kein besonderer Wert gelegt wurde." I have elsewhere remarked that it seems unwise to refer so great a variation in the observed zero point of the thermometer to accidental impurities of the distilled water used, since these impurities would need to be so great as to raise the water to the order of a $\frac{1}{1000}$ normal salt solution.

The series furthermore shows that the zero point of his thermometer was *falling*. During the six years that I have been observing the zero point on my thermometor, the zero point has been rising, whether it was in use or at rest. This is evident in every series except that of 1893, where the experimental errors were so great as to mask this rise of the zero point. I am persuaded that the greater part of these observed variations in the zero point of the thermometer are due to faulty methods.

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[1] Zeit. Phys. Chem., XX., 216.

The record of zero points for the series 1897, 1898 and 1899 is here submitted.

Date.	Observed zero.	Barometer.	Room Temp.	Zero point corrected to 760 mm. and 0°C.	Depressions observed on the given zero.
1897					
Feb. 26	0°.0510	761	2°C.	0.0500	Butyric Acid.
" 27	0.0517	769	2	0.0494	Propyl Alcohol.
Mar. 7	0.0512	773	1	0.0486	Cane Sugar.
" 16	0.0518	771	2.5	0.0490	Ethyl Alcohol.
" 28	0.0505	759	3	0.0495	Normal Butyl Alcohol.
Apr. 21	0.0521	773	3.5	0.0506	Control for Sugar, M = 0.10.
1898					
Feb. 26	0.0520	760	3	0.0508	Part of Methyl Alcohol.
Mar. 31	0.0542	760	5	0.0522	Urea.
Dec. 29	0.0545	759	2.5	0.0537	Part of Chloral Hydrate.
1899					
Jan. 2	0.0569	781	1	0.0530	Chloral Hydrate and Acetone.
" 3	0.0558	773	1	0.0532	Ether and Mannite.
" 7	0.0552	763	2	0.0540	Urea.
" 10	0.0562	771	2	0.0536	Control for Urea.
" 19	0.0566	766	4	0.0541	Amyl Alcohol.
" 25	0.0555	750	3	0.0550	Part of Amyl Alcohol.
" 28	0.0557	760	2	0.0549	Aniline and $MgSO_4$.[1]
" 30	0.0556	759	2	0.0550	H_2SO_4[1] and part of $MgSO_4$.[1]
" 31	0.0551	757	2	0.0548	No depressions measured.
Feb. 1	0.0566	766	2	0.0548	NaCl.[1]
" 11	0.0570	771	2	0.0544	Acetamide.[1]
Mar. 18	0.0556	757	4	0.0545	Control for Acetamide.

These zero points are represented graphically in Fig. 1.

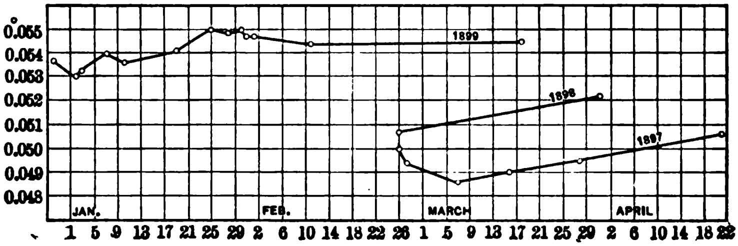

Fig. 1.

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[1] Results reserved for a subsequent paper.

It needs to be stated that the zero point at the end of the 1896 series was 0.°045. Thus the slow rising of the zero point which has been observed before is still going on, though the rate is noticeably less. It is worth noticing that the variation in the zero point is such as to be largely accounted for by the well-known changes which a thermometer undergoes—a slow rise of the zero point. The exact uniformity in the rate of this elevation of the zero point for the greater portion of the 1897 period is certainly not other than accidental, but the very nearly uniform rise in the first part of the 1899 period and its almost exact constancy during the latter portion of the same period, are clearly indicative of the high degree of exactness which the method has attained.

The range of variation for the 1897 series is seen to be 0°.002 C. in six zero points, and in the 1899 series it is seen to be 0°.002 in thirteen zero points, extending through a period of three months.

In each case the greater part of this variation is accounted for by the obvious fact that the zero point has been rising. The remainder of the variation I think represents the experimental errors.

It is not without interest to observe that this thermometer has experienced a total rise of the zero point during the six years that it has been in use since it was corrected at the Reichsanstalt, amounting to not less than 0°.023 C. The rate has been about 0°.0003 C. per month. The rise during the period of use is noticeably more rapid than this.

Further, any attempt to judge the exactness of any method must take notice of the facts which Nerst and Abegg[1] so clearly set forth in connection with their study of the disturbances which the equilibrium temperature of a mixture of water and ice undergoes on account of the temperatures of the surrounding bodies and the heat developed by the stirring. In the statement of their results they have introduced a temperature called the convergence temperature, which may be defined as that temperature which the water *without the ice* would assume when it has come into thermal equilibrium with its surroundings. They showed that in case the conditions of the experiment were not so chosen that this convergence temperature was identical with the true temperature of equilibrium of ice and water, then the observed temperature of the mixture when it has come into thermal equilibrium with its surroundings is not the true temperature of equilibrium of the ice and water sought, and that in certain cases the measurement of depressions of freezing points would be affected by grave errors unless the observations were corrected in a manner shown by the authors.

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[1] Zeit. Phys. Chem., XV., p. 681.

They conclude that the observed freezing point of any liquid is never its true "freezing point" unless one of two conditions are met; either the convergence temperature must be coincident with the true freezing point, or the *rate* at which a liquid comes to temperature equilibrium when once it begins to freeze must be infinitely great. This may be accepted without hesitation. Then they express themselves in these words:

"Since the former condition has never yet been exactly fulfilled, it has been assumed in the earlier investigations that the equilibrium between the solid and liquid solvent takes place at an infinite velocity."[1]

I have elsewhere said that the "convergence temperature" played no rôle in the method which I have used.[2] From the very beginning of the work the disturbance which the temperature of a mixture of ice and water suffers on account of the temperature of its surroundings and the heat due to the stirring was well known to me, and the entire development of the method was always in the direction of eliminating these disturbances. Thus in the first brief notice of the work[3] I express myself on this point as follows:

"A fundamental difficulty lies in the fact that the temperature of a mixture of ice and water is influenced by the temperature of the surroundings, * * * * and a further difficulty consists in the fact that this influence, which is caused by the heat exchanges between the mixture and its surroundings, *and also by the heat developed by the stirring*, varies with the quantity and fineness of the ice present. In order thus that the temperature of a mixture of ice and water may be as nearly as possible the (true) freezing point (of the water), that is, the temperature at which the ice and water alone would be

[1] Zeit. Phys. Chem., XV., p. 683. 1894.
[2] Jour. Phys. Chem., I., p. 224.
[3] Ber. Berichte, 1893, XXVII., p. 798.

in equilibrium, it is necessary that these disturbing influences be eliminated." In the fuller publication of the work[1] stress is laid upon the success with which this was accomplished, aud data are then presented which enabled one to conclude that "the disturbing influences of the freezing bath and surrounding air have been so far eliminated that the *actual* freezing point of water, and so too of a solution, may be determined." This conclusion was drawn from a series of observations on the freezing point of water in which about one-half the individual observations were made with very large amounts of ice present, while the remainder were made with only traces of ice present. It was found that the average values of these two portions of the series were the same. But Abegg[2] reminds me that my conclusion that the convergence temperature was thus shown to be identical with the true freezing point under observation does not follow necessarily. Since, as he says, the same result would be found if the rate at which a mixture of ice and water comes to its equilibrium temperature is very great. The theory requires this rate to be infinitely great. Now simply put, this rate depends upon nothing save the rate at which the ice melts or the water freezes, and there is little need to talk about this rate being "very great" to say nothing about its approaching an infinite value. It is necessary to observe that my conclusion that the "convergence temperature" of my apparatus was coincident with the true freezing point of the solutions was the result of long experimental work which had no other aim than to bring about this essential result, and I have never had the slightes ground for doubting the soundness of my conclusion. It is for this reason that I have not thought it necessary to reopen this part of the investigation. But Abegg has kindly urged upon me the value of some direct test of the matter, and on March 7, 1899, I made the following experiment:

Room temperature 3°.5 C., barometer 742 mm. The observed freezing point of water was 0°.0520. To find the convergence temperature of the apparatus it is only necessary to proceed exactly as though a determination of the freezing point of the water were to be observed except that the water is not "touched off," so that the water comes to thermal equilibrium with no ice present. After a few minutes the thermometer showed 0°.056, and this was the final reading after ten minutes of stirring. Thus the convergence temperature of the apparatus when the air is at 3°.5 C. and the protection bath is 0°.3 C. is 0°.004 C. above the true freezing point of water. This difference between the convergence temperature of the apparatus and the freezing point of the water under observation is many times smaller than is necessary to insure the exactness of the method. The result of this experiment is exactly what I had every reason more than six years ago to believe was the fact: The convergence temperature plays no rôle in the results obtained by use of the present method. It should be added that sufficient uniformity in the stirring is had by keeping time to a "sing-song" the exact words of which have little scientific interest.

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[1] Phys. Rev., 1893, I., p. 213.

[2] Wied. Ann., 64, p. 490.

TABLE I.

1 Compound.	2 m	3 Grams per liter of the solution.	4 Sp. gr. of the solution in (3) $\frac{18^\circ}{4^\circ}$	5 Sp. gr. of the solution $m = 0.20$.	6 Source of the material.	7 Manner of Preparing Solution in (3).
Methyl Alcohol.	1.	32.02	0.9928	0.9975	Prepared in Lab.	By direct weight, after drying.
Ethyl Alcohol.	1.	46.04	0.9901[1]	0.997	Squibb's 99.89%.	" " "
n-Propyl Alcohol.	1.	60.05	0.9894	0.9967	Kahlbaum.	" " " " "
n-Butyl Alcohol.	1.	74 .06	....	0.997[7]	Kahlbaum.	" " "
Amyl Alcohol.	0.1	8.81	.....	0.997[2]	Kahlbaum.	" " " " "
Glycerine.	1.	92.04	1.0203	1.003	Eimer & Amend.	Gerlach's Tables.[3]
Dextrose.	0.9834	177.08	1.0664[4]	1.0125[4]	Merck & Co.	Fresenius' Tables.[5]
Cane Sugar.	1.	342.1	1.1294	1.024	Rock Candy.	By direct weight, after drying.
Mannite.	0.5	91.04	1.0304	1.012	Merck & Co.	" " " " "
Ether.	0.5	37.03	0.9911	0.996	Squibb's Abs.	" " " " "
Acetone.	1.	58.04	0.9908	0.997	Kahlbaum.	Squibbs' Tables.[6]
Chloral Hydrate.	1.	165.4	1.0722	1.0159	Kahlbaum.	Direct weight.
Aniline.	0.2	18.61	....	0.9995	Kahlbaum.	" " after drying.

[1] Sp.gr., $\frac{20^\circ}{4^\circ}$.

[2] $m = 0.10$.

[3] Landolt and Börnstein Tabellen, 1895, p. 230.

[4] Sp. gr., $\frac{17.5^\circ}{4^\circ}$.

[5] Zeit. Anal. Chem., 1883, XXII., p. 454.

[6] Ephemeris, VII., p. 541.

[7] Assumed.

The present investigation includes the non-electrolytes, which are tabulated with the usual accompanying data in the opposite table.

METHYL ALCOHOL, 1896.

The solution $m = 1$ was prepared from methyl alcohol which had been made by the saponification of methyl oxalate crystals with ammonia water in a manner which is described below. The tables of specific gravity furnished by Dittmar and Fawsitt[1] were used. But the specific gravity of the resultant normal solution was 0.99313 at 15° 56C. and the solution according to these tables was thus 5 % too strong.[2] The observed depressions were accordingly corrected by this amount, reducing the observed molecular depressions to the almost constant value 1.73. This result was so surprising that it could not be accepted until all possibility of error in the concentration of the solutions was set aside. To this end nothing would avail except the attempt to prepare *pure methyl alcohol.* This I undertook to do. The work, however, was so great and was so slow of accomplishment that the publication of the large part of the experimental material of the years 1896, 1897 and 1898 had to be delayed to the present time. This will account for the confusion in the dates of the various series of observations. To prepare methyl alcohol the following method was used: The best methyl alcohol of commerce is converted into methyl oxalate by adding 800 cc. of the alcohol to a solution of 1000 gr. oxalic acid crystals in 400 cc. of sulphuric acid. When this is allowed to stand in a cool place the methyl oxalate separates out in an abundant crop of colorless crystals, which are drained and *washed with ice cold water.* The product has a marked "mouse nest" odor. The crystals are now digested with a strong solution of ammonia in water, using a glass flask provided with an "inverted condenser." This leads to the saponification of the methyl oxalate with the regeneration of methyl alcohol. It is then distilled and all of the distillate collected that will come over from a bath kept at 100° C. The product is about half water, the greater part of which is removed by the use of dry K_2CO_3 and

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[1] Edinburgh Trans., XXXIII., p. 509.

[2] In the Princeton Bulletin, May, 1896, in publishing the value for a .1 normal methyl alcohol solution I used a correction factor of 3 per cent. by mistake.

CaO in the order named. The yield is scanty, and large quantities of the materials have to be employed, for the product must again be put through the oxalate process.

After this has been done the methyl oxalate has lost its odor. The resultant methyl alcohol is dried as before and then subjected to a long series of successive distillations from metallic sodium. The distillations were made backwards and forwards between two distillation flasks, each of which served in turn as distilling flask and condenser.

In this way no pouring of the alcohol from one vessel to another was necessary. Between each distillation the specific gravity of the alcohol was observed. Some twenty such distillations from small bits of sodium were made before the specific gravity had been reduced to a constant value. I think much of this work would have been unnecessary if I had learned earlier in the work that methyl alcohol when nearly *dry* takes water from the air, in spite of ordinarily careful stoppering, so rapidly that its specific gravity goes up three or four points in the fourth place of decimals during a single night. When this fact became known the exclusion of the moisture of the air received the utmost care, and the determinations of the specific gravity were made in a pyknometer so arranged that the alcohol could be transferred to it from the condensing flask without contact with the air's moisture. In this way the specific gravity was reduced to a constant value.

At 10° the specific gravity = 0.80065.
At 20° the specific gravity = 0.79133.
Water at 4°C. = 1.
The boiling point of this methyl alcohol
At 766.74 mm. is 64°.70C.
At 752.92 mm. is 64.23.
Thus at 760 mm. it is 64°.5C. (Gasthemometer, Reichsanstalt).

It needs to be observed that the boiling point was taken as usual with the entire stem exposed to the vapor and the bulb about 4 cm. above the liquid. A quantity of glass beads was put in the flask to insure uniform boiling. The entire product, except the last frothing traces, went over at the temperatures given above. I find that the slight rise of 0°.1—0°.2 which occurs at the last moment is no indication of an impure product, but results wholly from the unavoidable "overheating" when any small quantities of the substance are in the flask. Thus I find that the *purest water* behaves in the same way.

While the specific gravity and boiling point of methyl alcohol have thus been very much lowered below the hitherto accepted values, I have no reason for thinking that a *dry* product has yet been obtained. Only the limit which the apparatus and drying method used in the present work have fixed has been reached. It is worth mentioning that I find that sodium continues to dry methyl alcohol long after dry copper sulphate and P_2O_5 have ceased to act. It is probable, then, that Carrara,[1] in his attempt to measure the dissociation of water in methyl alcohol did not succeed in getting a *dry* alcohol as the basis of his work.

The methyl alcohol prepared in the way described above *has lost all traces of its characteristic odor.* I am of the opinion that a slight odor still remains, though some who have examined the product do not admit this. The details of this purely incidental work will perhaps be given at some future time in case opportunity arises to measure some other important constants of pure methyl alcohol, some 40 grams of which I still have left.

From this alcohol a gram-molecular solution was made by direct weighing. Its specific gravity at 18° C. was found to be 0.99281 and at 15°.56 C. it was 0.99318. Thus my suspicion that the tables of Dittmar and Fawsitt are faulty in the region of great dilution is confirmed. Using this final normal solution I then prepared a tenth normal solution for the purpose of determining its freezing point as a control to the observations made in 1896. The result was exactly 1% higher than the value found in 1896, and I should accordingly correct the entire series by this amount. But unfortunately a confusion exists in the original notes which forces me to regard the specific gravity of the normal solution prepared in 1896, as probably wrong, so that I am unable to say as I should like to that the 1896 solution was 1% weaker than the final one used in the "control." I can do no more than to give the 1896 series and say that they may be 1% too small. At the first opportunity I shall repeat the entire series.

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[1] Gazz. Chim. Ital., 1897, XXVII., p. 1.

Methyl Alcohol, Feb. 17, 1896.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	0°.0183	1.83	0.321	0.01	1.83
0.02	0.0362	1.81	0.641	0.02	1.81
0.03	0.0548	1.83	0.963	0.0301	1.82
0.05	0.0910	1.82	1.605	0.0502	1.81
0.10	0.1818	1.82	3.216	0.1005	1.81
0.20	0.3655	1.83	6.457	0.2018	1.81

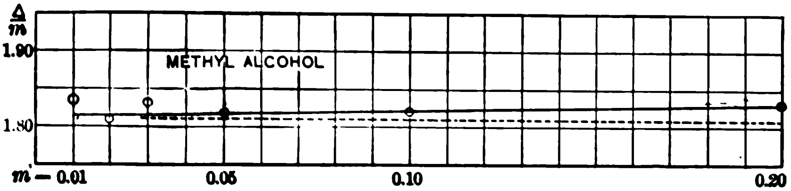

Fig. 2.

In the tables throughout this paper m denotes the number of gram-molecules per liter of the solution ; $\triangle$, the depression of the freezing point, and $\triangle/m$ is what is generally known as the *molecular depression.*

By the help of the specific gravity of the solution $m = 0.20$, it is easy to reduce the values of m to the values m' which denote the number of gram-molecules per 1000 grams of water in the solution. To this end the calculated number of grams of the substance per 1000 grams of water is given under P in the tables. m' equals of course the quotient of P divided by the molecular weight.

The graphic representation of the results is presented with each table, using my usual method, in which the ordinates represent the the values of $\triangle/m$, and the abscissas the values of m. The dotted line gives the values of $\triangle/m'$.

The "curve" turns out to be a straight line. The observed values do not in any case differ from those that such a straight line would require by more than 0°.0003 C. The molecular depression is thus seen to be 1.82 in extreme dilution, increasing slowly to 1.83 in the region of greater concentration.

ETHYL ALCOHOL, MARCH 16, 1897.

The solution, $m = 1$, was made by direct weight from Squibb's "absolute alcohol." Dr. Squibb was kind enough to determine the specific gravity of the product which he sent me directly before the can was sealed. He found specific gravity $\left(\frac{15^\circ.6}{15^\circ.6}\right)$, 0.79386. According to his table[1] the strength was 99.89%. The solution $m = 1$ had a sp. gr. $\left(\frac{20^\circ}{4^\circ}\right)$ 0.99005.

Ethyl Alcohol, March 16, 1897.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	$0^\circ.0181$	1.81	0.460	0.01	1.81
0.02	0.0368	1.84	0.921	0.02	1.84
0.05	0.0920	1.84	2.312	0.0502	1.83
0.10	0.1849	1.85	4.633	0.1007	1.84
0.20	0.3707	1.85	9.314	0.2024	1.83

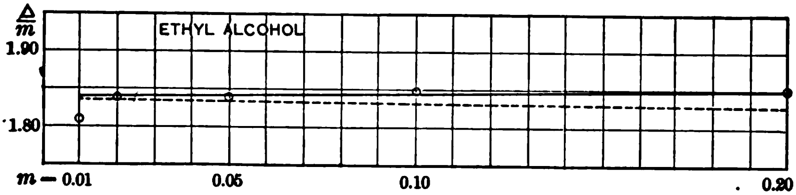

Fig. 3.

The comparison of these results with those of 1892[2] is instructive. For this purpose the earlier results are given below:

Ethyl Alcohol, Dec., 1892.

m	Δ	$\frac{\Delta}{m}$	Corrected Values.
0.01	$0^\circ.0172$	1.72	1.74
0.02	0.0351	1.76	1.78
0.04	0.0729	1.82	1.84
0.05	0.0894	1.79	1.81
0.10	0.1825	1.83	1.85
0.16	0.2927	1.83	1.85

Before making the comparison it must be remembered that the solutions of 1892 were known to be 1% too weak (see note, p. 282,

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[1] Ephemeris, II., p. 541.

[2] Phys. Rev., I., p. 199.

l. c.). Applying this correction to the series we get the values shown in the last column of the table. The values at $m = 16$, $m = 0.10$ and $m = 0.04$ are identical with those in the new series. The values at $m = 0.01$, $m = 0.02$ and $m = 0.05$ fall below those of the new series. This comparison of the two series furnishes a very striking confirmation of the conclusion which has been reached in regard to the error which arose in the earlier work on account of packing the whole thermometer in ice when out of use. The zero point is therefore uniformly made too low and the curve of molecular depressions, which was found to be straight in the region of greater concentration, is made to curve considerably downward in the region of great dilution where the error in the zero point would be most felt. A second matter of interest appears in the very singular value for $m = 0.04$. In the paper of 1892, this observation is looked upon as incorrect only because it was so out of accord with the rest of the series, and I was forced to assume the possibility of an error amounting to 0°.0015 C., an error which, apart from this single observation, I had no right to admit.

It now appears that this one observation was the only one that was right in the region of extreme dilution. The observations at that time required generally a whole day for the determination of the zero point of the thermometer and the freezing point of the solution, so that it is easy to believe that this single determination could have been so made as to escape the influence of the usual source of error.

As in the case of methyl alcohol the graphic representation of the results is best effected by a *straight line* which shows, allowing for errors in no case greater than 0°.0003, that the molecular depression is *1.84 in extreme dilution,* and that it increases uniformly to 1.85 at $m = 0.20$.

It is a matter of very great importance as well as gratification that the last results of Raoult,[1] are so exactly in accord with these values that it is impossible to distinguish the two series when graphically represented on the scale of the present curves. Although my work was done some time before Raoult published his results, Raoult had no knowledge of it until after the publication of his work. The two series have been made by different observers in entire independence and *without the guiding hand of any theory* and must be looked upon as affording the most strikingly confirmation of each other.

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[1] Zeit. Phys. Chem., XXVII.

PROPYL ALCOHOL, FEBRUARY 27, 1897.

The solution $m = 1$ was made by direct weight from Kahlbaum's product. The product was successively distilled from sodium as in case of methyl alcohol, though with much less care. The boiling point of the middle portion of a final distillate was constant within 0°.1 C.

The specific gravity of this portion, at 15° C., was 0.80798; at 20°C., 0.80406.

Its boiling point at 760 mm. is 97°.2 C. These values, as with methyl alcohol, are much lower than those hitherto accepted, due simply to the greater dryness of the present products.

n-Propyl Alcohol, Feb. 27, 1897.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	0°.0189	1.89	0.601	0.01	1.89
0.02	0.0371	1.86	1.201	0.02	1.86
0.05	0.0936	1.87	3.015	0.0502	1.86
0.10	0.1858	1.86	6.015	0.1008	1.84
0.20	0.3723	1.86	12.191	0.2031	1.83

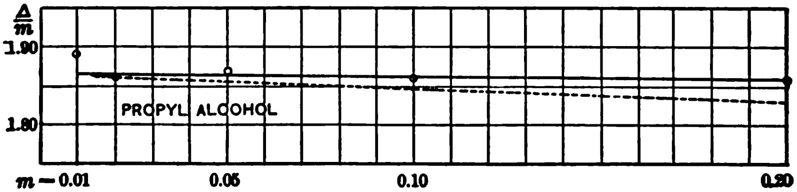

Fig. 4.

The graphic representation again is best effected by a straight line. The errors which must be assumed in drawing this conclusion do not exceed 0°.0003C.

The molecular depression is seen to be 1.87 in extreme dilution and to fall uniformly to 1.86 in the region of greatest concentration.

n-Butyl Alcohol, March 28, 1897.

The solution $m = 1$ was made by direct weight from Kahlbaum's product. The quantity at my disposal was so small that no drying was possible. I assumed that there was 1 % water present and made allowance for this in making the weighings. Its specific gravity at 20° with a Mohr balance was 0.8095 ± 0.0002. An attempt to dry a sample obtained afterwards from the same source, with the hope that a "control" determination of the freezing point would enable one to "correct" the original series resulted in failure; since, in spite of the utmost care in the successive distillations from Na, the product did not show a constant boiling point within 5° C. I am unable to find that *n*-butyl alcohol has ever been prepared with such a degree of purity that the boiling point is any more constant than this, and I abandoned the idea of making any control determinations. I found the specific gravity of the product when dried with Na to be 0.8082 at 20° C. What degree of error there may be in the given values of the various concentrations I have no means of knowing. That the results are in accord with the rest of the alcohol series, of course, furnishes no ground for any conclusion in this matter.

n-Butyl Alcohol, Mar. 28, 1897.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	0°.0189	1.89	0.741	0.01	1.89
0.02	0.0368	1.84	1.482	0.02	1.84
0.05	0.0926	1.85	3.72	0.0503	1.84
0.10	0.1863	1.86	7.47	0.1009	1.85
0.20	0.3723	1.86	15.08	0.2037	1.83

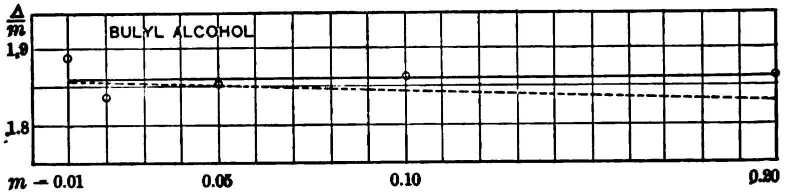

Fig. 5.

Again the graphic representation is a straight line. This conclusion requires the assumption of an error in no observation exceeding

0°.0003 C. The molecular depression in extreme dilution is 1.86 and this is the value also for all the concentrations.

Amyl Alcohol, February 27, 1897.

The original product was Kahlbaum's "amyl alcohol" and is the ordinary amyl alcohol of fermentation. It was dried with Na. The final product had a boiling point constant within 1°.1 C. Its specific gravity was 0.8121 at 18° C. The solubility of amyl alcohol in water does not permit the solution $m = 0.20$.

Amyl Alcohol, Feb. 27, 1897.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	0°.0185	1.85	0.883	0.01	1.85
0.02	0.0372	1.86	1.766	0.02	1.86
0.05	0.0926	1.85	4.431	0.0503	1.84
0.10	0.1845	1.845	8.922	0.1013	1.82

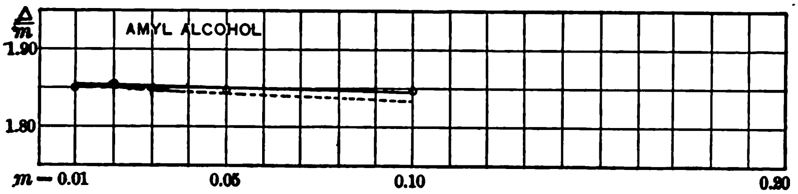

Fig. 6.

The graphic representation is again a straight line, with practically no error in any observation. The molecular depression in extreme dilution is 1.86 and this falls to 1.84 at $m = 0.10$.

Glycerine, February 13, 1896.

The solution $m = 1$ was made by use of Gerlach's tables of specific gravity.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	0°.0186	1.86	0.922	0.01	1.86
0.02	0.0372	1.86	1.845	0.0201	1.85
0.05	0.0929	1.86	4.622	0.0502	1.85
0.10	0.1869	1.87	9.280	0.1008	1.85
0.20	0.3758	1.88	18.688	0.2031	1.85

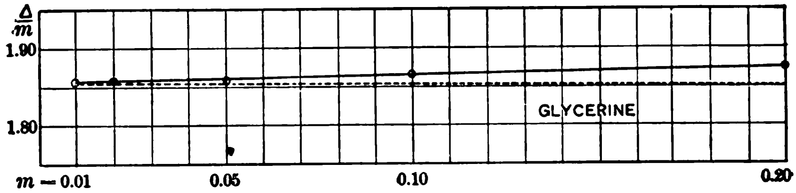

Fig. 7.

The graphic representation is a straight line, and this conclusion requires no appreciable errors to be admitted. The molecular depression is 1.86 in extreme dilution and rises to 1.88 at $m = 0.20$.

Dextrose, Feb. 22, 1896.

The product was from Merck and Co. It was not true to their label in regard to dryness, and so an approximate solution, $m = 1$, was made by weight and then corrected by use of the Fresenius tables.[1] The specific gravity of the approximate solution was 1.0664,

Dextrose, Feb. 22, 1896.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.0098	0.0185	1.88	1.774	0.0098	1.87
0.0197	0.0363	1.84	3.559	0.0198	1.84
0.0492	0.0916	1.86	8.927	0.0496	1.85
0.0983	0.1850	1.88	17.95	0.0997	1.86
0.197	0.3745	1.90	36.30	0.2016	1.86

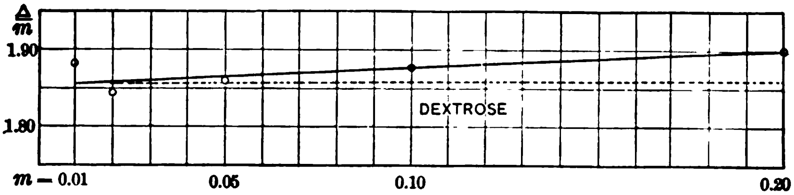

Fig. 8.

and was thus 1.66% too weak according to these tables. The concentrations in column (m) are corrected by this amount.

A true $m = 1$ solution, prepared from Kahlbaum's fine crystalline product, when freed from its bare trace of water by most careful drying, has been found to have a specific gravity at $\left(\frac{17.5}{17.5}\right)$, 1.0684 and at $\frac{18^\circ}{4^\circ}$, 1.0669. This is so nearly in accord with the Fresenius tables that I have accepted them with confidence.

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[1] Zeit. An. Chem., XXII., p. 454.

The graphic representation is again a straight line, a conclusion which requires the admission of experimental errors reaching to 0°.0004 C.

The molecular depression is 1.85 in extreme dilution and rises to 1.90 at $m = 0.20$.

CANE SUGAR, MARCH 7, 1897.

The solution $m = 1$ was made by direct weight from carefully selected crystals of "rock-candy." These were pulverized and dried over sulphuric acid. The solutions were made immediately before the observations on their freezing points.

Cane Sugar, March 7, 1897.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	0°.0187	1.87	3.432	0.01	1.87
0.02	0.0378	1.89	6.879	0.0201	1.88
0.05	0.0947	1.89	17.312	0.0506	1.87
0.10	0.1918	1.92	34.962	0.1022	1.88
0.20	0.3960	1.98	71.599	0.2093	1.89

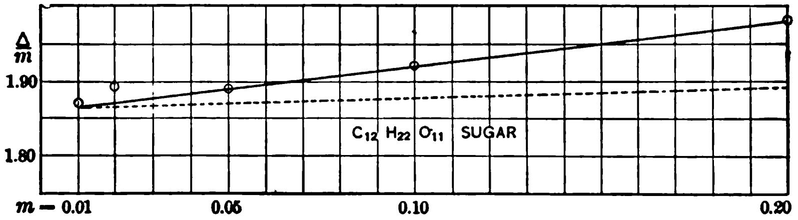

Fig. 9.

The graphic representation is again a straight line if an error of 0°.0004 be assumed at $m = 0.02$.

The molecular depression in extreme dilution is 1.86. It rises rapidly to 1.98 at $m = 0.20$.

These results should be compared with the series of 1892. It appears that, barring the downward curvature in the region of extreme dilution, which all the early series exhibited, the new results are exactly 1 per cent. higher than the old. The solutions however were not of the same strength, as appears from their specific gravity. Thus the solution $m = 1$, at 17°.5 C., has a specific gravity at 18°, 1.1294. The solution used in 1892 ($m = 1$ at 4° C.) had a specific gravity at 18°, 1.1285.[1] Accordingly, using the tables of specific gravity in the Kohlrausch Leitfaden, eighth edition, the old solutions were 0.6 per cent. too weak. The two series then are *parallel* and differ by 0.4 per cent., a difference probably less than the experimental errors of the method.

The latest values found by Raoult[2] are exactly parallel with these results when they are computed on like definition of a gram-molecular solution, but are exactly 1 per cent. higher. The agreement, while falling short of complete identity, as in the case of ethyl alcohol, is still not less striking, since the two series agree in that they confirm each other in the *fact of a uniform increase in the value of the molecular depression* with increase in the concentration. It is to be observed that the results of Raoult are expressible by a straight line without the smallest trace of an error, say at $m = 0.03$, when the error reaches 0°.0003 C. It is to be regretted that he did not examine solutions more dilute than $m = 0.03$, since his method clearly would justify this.

The two series are represented graphically in the annexed figure, in which m is the number of gram-molecules per 1000 grams of water (m' in the tables).

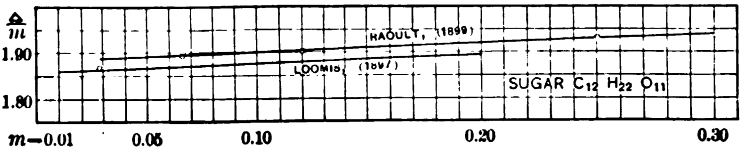

Fig. 10.

MANNITE, JANUARY 3, 1899.

The solution $m = 1$ was made by weight from Merck's product after drying at 100°C. to constant weight.

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[1] The published value was 1.1286. This was reckoned without making allowance for air displacement, which is now done.

[2] Zeit. Phys. Chem., XXVII., p. 617.

Mannite.

m	Δ	$\frac{\Delta}{m}$	p	m'	$\frac{\Delta}{m'}$
0.01	0°.0185	1.85	1.821	0.01	1.85
0.02	0.0371	1.86	3.656	0.0201	1.85
0.05	0.0931	1.86	9.169	0.0504	1.85
0.10	0.1874	1.87	18.44	0.1013	1.85
0.20	0.3807	1.90	37.33	0.2061	1.85
0.50	0.9835	1.97	96.92	0.5323	1.85

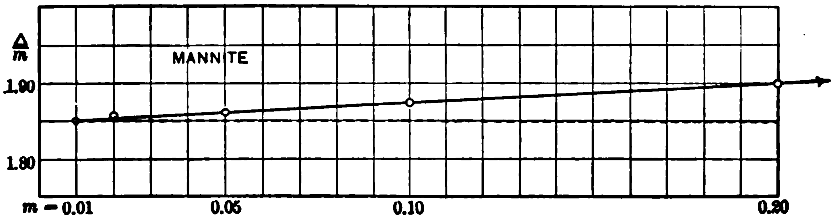

Fig. 11.

The results are again in a straight line if an error of 0°.0005C. be assumed in the observations at $m = 0.10$ and $m = 0.20$. The molecular depression is 1.86 in extreme dilution. It must be remarked that the results in the present case could be expressed by a curve slightly convex on the upper side and given a value for the molecular depression 1.84 in extreme dilution. I am of the opinion, however, that this is due to the distribution of the experimental errors in spite of the fact that an error of 0°.0005C. has to be admitted to make the results expressible by a straight line.

ACETONE, JANUARY 2, 1899.

The solution $m = 1$ was made from Kahlbaum's "Aceton aur der Bisulfitverbindung," by use of Squibb's tables of specific gravity.

Acetone.

m	Δ	$\frac{\Delta}{m}$	p	m'	$\frac{\Delta}{m'}$
0.01	0°.0185	1.85	0.580	0.01	1.85
0.02	0.0372	1.86	1.163	0.02	1.86
0.05	0.0932	1.86	2.914	0.0502	1.86
0.10	0.1846	1.85	5.851	0.1008	1.83
0.20	0.3706	1.85	11.78	0.2030	1.83
1.00	1.914	1.91	62.22	1.072	1.79

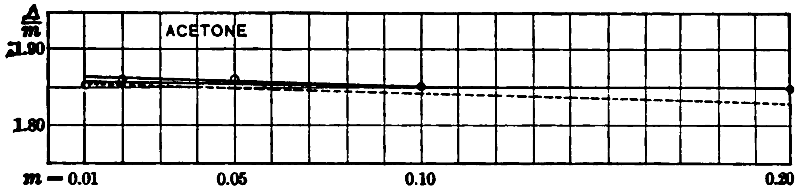

Fig. 12.

The representation of the *entire* series by a straight line is impossible. A curve, with its convex side downward, beginning at 1.86 in extreme dilution and running nearly straight in the region between $m = 0.01$ and $m = 0.20$ represents the results best. The errors necessary to this conclusion are inappreciable, while a straight line would require errors of 0°.001 at $m = 0.20$, and 0°.007 at $m = 0.05$.

The molecular depression is again 1.86 in extreme dilution and rises to 1.91 at $m = 1$.

CHLORAL HYDRATE, JANUARY 2, 1899.

The solution $m = 1$ was made from Kahlbaum's product by direct weight, after exposure to sulphuric acid. The powdered crystals are volatile and it is impossible to bring the compound to a constant weight.

Chloral Hydrate.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	0°.0186	1.86	1.655	0.01	1.86
0.02	0.0373	1.87	3.318	0.0201	1.86
0.05	0.0924	1.85	8.315	0.0503	1.84
0.10	0.1875	1.87	16.696	0.1009	1.86
0.20	0.3760	1.88	33.66	0.2035	1.85
1.00	2.050	2.05	182.40	1.1028	1.86

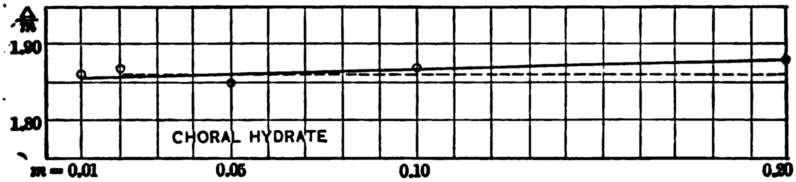

Fig. 13.

The graphic representation of the *entire series* shows that the results cannot be expressed by a straight line unless an error of more than 0°.001 be assumed at $m = 0.10$ and $m = 0.20$. As I have no right to assume so large an error, it must be said that here also a curved line with slight convexity on its lower side and practically straight in the region between $m = 0.01$ and $m = 0.20$, best represents the results. The necessary error for this conclusion is 0°.0005 C. in the observation $m = 0.05$.

The molecular depression is 1.86 in extreme dilution and rises to 2.05 at $m = 1$.

ANILINE, JANUARY 28, 1899.

Kahlbaum's product was distilled twice from sodium and made into a solution $m = 0.20$ by weight. The specific gravity of the aniline after the distillation was, at 15 C., 1.0260 and at 20° C., 1.0215.

Aniline.

m	Δ	$\frac{\Delta}{m}$	P	m'	$\frac{\Delta}{m'}$
0.01	0°.0185	1.85	0.931	0.01	1.85
0.02	0.0372	1.86	1.867	0.0201	1.85
0.05	0.0914	1.83	4.681	0.0503	1.82
0.10	0.1811	1.81	9.407	0.1011	1.79
0.20	0.3549	1.77	18.97	0.2038	1.74

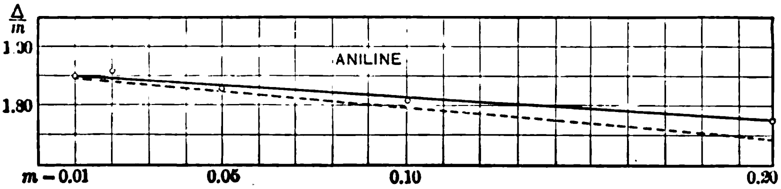

Fig. 14.

The results lie in a straight line, with an assumed error of 0°.0005 at $m = 0.10$.

The molecular depression is 1.86 in extreme dilution and falls rapidly to 1.77 at $m = 0.20$.

ETHER, JANUARY 3, 1899.

Squibb's "absolute ether" was distilled twice from sodium. It was then found to be no longer acted on by this drier. Its specific gravity at 15° C. was 0.7193, and its boiling point was *34°.3* C. at 760 mm. These values are much lower than the accepted values for these constants, save only the specific gravity, as found by Squibbs,[1] namely, 0.7189 at 15°.

Ether.

m	Δ	$\frac{\Delta}{m}$	p	m'	$\frac{\Delta}{m'}$
0.01	0°.0162	1.62	0.741	0.01	1.62
0.02	0.0336	1.68	1.486	0.0201	1.67
0.05	0.0856	1.71	3.724	0.0503	1.70
0.10	0.1734	1.73	7.486	0.1011	1.72
0.20	0.3468	1.73	15.10	0.2038	1.70

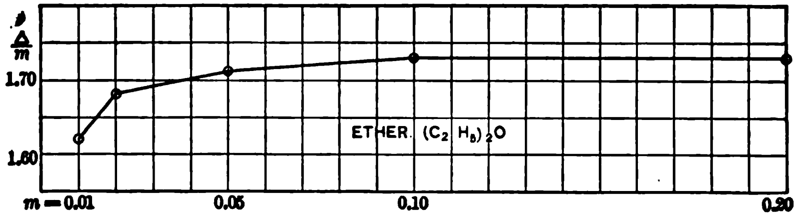

Fig. 15.

The graphic representation is a curve line with a pronounced convex side on the upper side. The molecular depression in extreme dilution can be obtained only by a very uncertain extrapolation which gives, say, the value *1.50*. The molecular depression becomes constant at $m = 0.10$, with the value 1.73.

General Discussion.

It is well known how van't Hoff has shown that the freezing point of a solution of any non-electrolyte in any solvent is theoretically capable of calculation by the formula,

$$\Delta = \frac{0.002T^2}{\lambda} \cdot \frac{G}{M}.$$

Where Δ denotes the difference between the known freezing point of the pure solvent and that of the given solution. T is this known freezing point of the solvent on the absolute scale, while λ is its heat equivalent of fusion. G denotes the number of grams of the solute per liter of the solution, and *M* is its molecular weight. It is convenient to call *G*/*M* the number of gram-molecules per liter, and to denote it by *m*.

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[1] Ephemeris, II., p. 678.

Thus in the case of aqueous solutions, using the value accepted by van't Hoff as the most probable for λ, we have, for the lowering of the freezing point due to any non-electrolyte:

$$\triangle = \frac{0.002(273)^2}{79} \cdot m,$$

and accordingly the value of the molecular depression is

$$\frac{\triangle}{m} = \frac{0.002(273)^2}{79} = 1.89.$$

This is the *van't Hoff constant.*

The early work of Raoult covered a large range of organic substances. The average value of the molecular depression of the *entire* series is as high as 1.85.[1] But the series comprises a large number of the *organic acids* which are known to be electrolytes.

Omitting these, since they have no right to be classed with the non-electrolytes, as everybody knows, the average value of the molecular depression of all the non-electrolytes examined by Raoult is 1.75, with aniline giving the value 1.53, and inverted sugar 1.93. This misrepresentation of Raoult's results is now quite general in the literature and for that reason I call attention to the matter.

More recent attempts to test the validity of the van't Hoff constant have been largely confined to ethyl alcohol, sugar and urea, which unfortunately constitute about the worst trio which could have been chosen for that purpose, since each presents an exception to the theory in question. Thus, sugar conforms to the theory only in solutions so dilute that the difficulty of determining the freezing point is very great. Ethyl alcohol does not conform to the theory at all, while urea seems to be so unstable in aqueous solutions that two attempts to repeat the work done by me on this compound in 1893 have been fruitless. For this reason no results for urea are found in the present paper.

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[1] Ostwald's Chem., II., p. 751.

DISCUSSION OF THE PRESENT RESULTS.

1. It is shown by the experimental results that a large number of widely different compounds in extreme dilution all have exactly *the same value for the molecular depression, namely 1.86.* This remarkable series comprises *n*-propyl alcohol, *n*-butyl alcohol, amyl alcohol, glycerine, acetone, dextrose, cane sugar, mannite and aniline.

The exceptions to this general conclusion are shown by

Methyl Alcohol,	1.82,
Ethyl Alcohol,	1.84,
Ether,	1.50.

These exceptions are well established by the experimental results.

Thus the van't Hoff requirement that the molecular depression for non-electrolytes in great dilution should be constant is fulfilled in a large range of compounds, *with the constant value 1.86.*

It is to be observed that the formula given by van't Hoff required the value 1.89. But the factor 0.002 which the formula contained is now known to be too high. It should be, according to present knowledge, 0.00198. With this correction the value of the van't Hoff constant would be 1.87, while the present experimental results require it to be 1.86. I believe that it will be found that the value for λ, namely the heat equivalent of fusion for ice, which appears in the formula and to which van't Hoff assigned the value 79 as the most probable will be found by experiment to be 79.3 such at least is the value required by the experimental facts presented in this paper, provided the exact validity of the van't Hoff constant is allowed.

2. It appears as the concentration of the various solutions increases, the greatest diversity in the values of the molecular depression comes to light.

Thus, in the case of cane sugar, the molecular depression has rapidly increased to 1.98 at $m = 0.20$; in other cases it has shown little change; while in others, noticeably aniline, the molecular depression has fallen rapidly to 1.77 at $m = 0.20$. The significance of this change in the value of the molecular depression is evident when we remember that the van't Hoff equation is deduced on the assumption that the dissolved substance behaves exactly as if it were in the state of an ideal gas with a volume equal to that of the solution. That is, it is assumed that the substance obeys the laws of Boyle and Gay Lussac *exactly*, which require $pv = RT$. Now no known gas does this perfectly. The familiar correction introduced by van der Waal by which the value of v is so corrected as to make allowance for the space actually taken up by the molecules themselves is known to render the product pv more nearly constant. The introduction of this correction in connection with a substance dissolved in any solvent is easily effected by defining the concentration of the solution not in terms of gram-molecules per 1000 cc. of the solution, but in gram-molecules per 1000 cc. of the *solvent* instead. This definition is the basis of the value, m', given in column 5 of each table. By referring to the tables, under the heading $\triangle/m'$, it is found that the molecular depression has become constant in the case of methyl and ethyl alcohol, glycerine, dextrose, mannite and chloral hydrate. In the light of the van't Hoff theory this amounts to saying that these substances in aqueous solution behave exactly as if they were in the state of a gas.

In the remaining cases the application of this correction to m does not suffice to render the molecular depression constant for all concentrations. In the case of sugar the correction seems to be too small to render the depressions constant, while in the rest of the cases this correction is too large to bring this about. The failure of this correction to bring the substances into accord with the requirement that they behave as gases is conspicuous in the case of aniline alone. It is the one case which I have examined where it seems possible to believe that the molecules of the dissolved substance combine themselves into molecular aggregates, more and more numerous in proportion as the concentration of the solution is increased. Anomalous as the aniline certainly is in the region of greater concentration it is the more important to observe that it is in exact accord with the demands of the theory when the dilution becomes extreme.

3. It will be remembered that the only exception to the general law that the molecular depression of non-electrolytes in *extreme dilution* is constant, was exhibited by methyl alcohol, ethyl alcohol

and ether. The exceptions here presented are pronounced and cannot possibly be explained by the assumption of unusual experimental errors. More experimental knowledge of these substances must be had before any explanation of their behavior in aqueous solution may be finally made. I wish merely to be permitted to suggest that the low values for methyl and ethyl alcohol is more than likely due to the fact that they evaporate rapidly from their aqueous solutions. To test this suspicion, I undertook to investigate the behavior of ether in this relation. The results confirm the suspicion, but exhibit an additional difficulty, namely, the values of the molecular depression are found not to increase or decrease uniformly with the increase in the concentration as is the case in the other compounds, but the change is according to a much more complex law. This appears best in the graphic representation. I have no explanation for this.

4. Before concluding this discussion of the results the important relation which they bear to the values found by the writer for the molecular depressions in the case of a large number of inorganic electrolytes must be pointed out. In general these values for the electrolytes have been about 2% lower than the dissociation theory of Arrhenius could account for. In making comparison between the experimental results and the requirements of this theory the van't Hoff constant plays a fundamental rôle, and I have always accepted the value 1.89 as the most probable theoretical value for this constant. According to the results of the present work it appears that this value is *too high* by 1.6%. Making use of the new value it turns out that the very large number of electrolytes which the author has examined are brought into full accord with this important generalization of Arrhenius.

SUMMARY.

1. The method of determining the freezing points of dilute solutions introduced by the author in 1893 is shown to conform to the fundamental requirement of all exact cryroscopy, that the convergence temperature of the apparatus must be coincident with the *true freezing* point to be observed.

2. A source of error in the early practice of the method has

been discovered. This error arose from the fact that in order to keep the thermometer when not in use at the temperature of 0° C., the whole instrument was packed in ice. It is now known that only the lower part should be so treated, the upper part should be kept always at room temperature. This error is shown to account for the slight downward curvature which the graphic representation of the early results for the non-electrolytes.

3. The method is used to find the molecular depressions of a large number of other non-electrolytes in aqueous solution. It is found that the van't Hoff constant is exactly verified in all the cases examined except methyl alcohol, ethyl alcohol and ether. The experimental value is found to be 1.86.

In conclusion I wish to say that a few remaining non-electrolytes which should be investigated before a general conclusion may be drawn in regard to the validity of the van't Hoff constant will be studied as soon as the opportunity presents itself, and it is hoped that the additional knowledge so acquired may throw light upon the exceptions already found.

PRINCETON UNIVERSITY, July 5, 1899.

AN ABSOLUTE DETERMINATION OF THE E.M.F. OF THE CLARK CELL.

By Henry S. Carhart and Karl E. Guthe.

A NEW determination of the electrochemical equivalent of silver was made a year ago by Patterson and Guthe[1]. The electrodynamometer employed for the purpose was fully described in their paper. Briefly a movable coil is suspended by a phosphorbronze wire approximately a meter long. The center of the movable coil is placed at the center of a fixed coil consisting of one layer of 576 turns of wire. The dimensions of both coils were intended to fulfill the requirements laid down by Andrew Gray[2] that the length should be as $\sqrt{3}$ to 1. The expression for the current is then

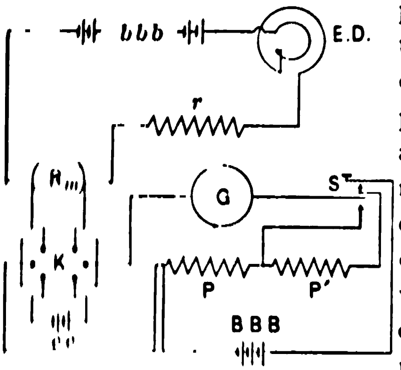

Fig. 1.

$$I = \frac{1}{T}\sqrt{\frac{2\pi K\sqrt{D^2+L^2}}{Nn}}$$

for a twist of the wire through an angle 2π. In this formula

T is the period of vibration of a weight M suspended by the phosphor bronze wire.

K, the moment of inertia of the mass M.

D, the diameter of the fixed coil.

L, the length of the fixed coil.

N, the number of turns of wire on the fixed coil.

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[1] Phys. Rev., Vol. VII, No. 6, Dec., 1898.

[2] Abs. Meas. in Electricity and Magnetism, Vol. II., p. 277.

n, the number of turns of wire on the movable coil.

r, the radius of the movable coil.

The electrochemical equivalent of silver found by Patterson and Guthe was 0.0011192 gm. per ampère per sec.

With such an instrument to measure the current, and with an appropriate coil of known resistance, it is obviously easy to compare the fall of potential of a measured current over this coil with the E. M. F. of a Clark standard cell. This work we have taken up the past year.

The Clark Cells.

Two cells were made in accordance with the specifications legalized by act of Congress in 1894. They are of the Rayleigh H-form as modified by Kahle. One of them was made in the autumn of 1897 and the other in October, 1898. They do not differ more than one part in ten thousand. The negative consists of a zinc amalgam one part zinc to nine parts mercury. The mercurous sulphate was washed as directed in the legal specification, and the zinc sulphate was rendered neutral with zinc oxide. The amalgam and the mercurous sulphate are covered with crystals of zinc sulphate to a depth of about one cm.

Each cell is mounted in a case the top of which is fitted with a bayonet joint and is supplied with four legs. When the hard rubber top is removed it carries the cell with it. The legs serve for a support in a bath of petroleum. The temperature was taken by a thermometer graduated to fifths. It was made by Haak, of Jena, and has the certificate number 11021 of the Reichsanstalt. At 15° it reads 0.05° too low as certified by the Reichsanstalt.

Method of Measurement.

The two Clark cells were connected in series. Their electromotive force was not balanced directly against the fall of potential over the manganin coil, but by Poggendorff's method the E. M. F. of the cells was first balanced against the fall of potential over a resistance P in Fig. 1, and then the fall of potential over the manganin coil R was balanced in the same way, while the current was adjusted so as to balance the torque of the phosphor-bronze wire of the electrodynamometer with a twist of one complete turn. Between the resistance boxes P and P', each of 10,000 ohms, was placed a shunted Kohlrausch cylindrical bridge wire whose 1000 divisions had a resistance of 1.7 ohms. This arrangement enabled us to balance to fractions of an ohm. The resistance in this circuit was thus kept at 10,000 ohms plus the resistance of the shunted bridge wire, the contact on which was adjustable to secure a balance. The resistance R over which the fall of potential was measured was the same manganin-coil used in the determination of the silver equivalent. The value was determined by comparison with two standard one-ohm coils made by Wolff in Berlin and tested by the Reichsanstalt. The certificates are numbered 160 and 163.

If I is the current measured by the electrodynamometer, E the E. M. F. of the two cells, R_1 the resistance in P necessary to balance E, R_2 the resistance to balance the P. D. due to I, and R the resistance of the manganin coil, then

$$E = RI\frac{R_1}{R_2}.$$

If I be put equal to a/T, in which a is made up of the constants in the expression for I already given, then

$$E = R\frac{R_1}{R_2}\cdot\frac{a}{T}.$$

The axis of the movable coil was placed at right angles to the magnetic meridian to avoid dip. Moreover, it was necessary to reverse the current through the stationary coil without reversing through the suspended coil so as to eliminate a small disturbance due to the manner of leading in the current to the movable coil. Hence it was necessary to use all four permutations of direct and reversed currents through the two coils.

Data and Results.

The following table gives the resistances necessary for a balance by the Poggendorff method of comparison. In arrangement 2 the current was reversed through the movable coil without reversal through the fixed coil as compared with 1. R_a is the resistance to balance with the current in one direction, R_b the resistance to balance with the current reversed through the entire instrument, and R_1 the resistance to balance the e. m. f. of the two standard cells. The column headed t is the temperature of the phosphor-bronze wire.

TABLE I.

Date.	Arrangement.	R_a	R_b	R_1	t	T
May 27.	1	7303.8	7299.0	4631.9	23.0°	11.9850
	2	7422.25	7418.9	4631.9	23.4	
" 30.	1	7324.5	7320.0	4644.3	22.9	11.9847
	2	7443.0	7440.7	4644.3	23.2	
" 31.	1	7329.5	7323.7	4647.5	24.25	11.9875
	2	7449.4	7445.0	4647.5	24.2	

The value of the resistance R to give a balance if there were no earth's field or asymmetry in the electrodynamometer would be very approximately $\sqrt{R_a'R_b'R_a''R_b''}$, where the primes refer to arrangement 1 and the seconds to arrangement 2.

The resistance of the manganin coil was found to be 4.6304 ohms at 23°. Its temperature coefficient is very nearly zero at this temperature. Three comparisons were made with the Wolff coils by the Carey Foster method. Two additional English one-ohm coils were first compared with the German coils, and then all four in series with the manganin coil.

Referring again to the formula for the electromotive force,

$$E = R\frac{R_1}{R_2}\cdot\frac{a}{T},$$

we have to find the constant

$$a = \frac{1}{r}\sqrt{\frac{2\pi K\sqrt{D^2 + L^2}}{Nn}}.$$

The constants of the electrodynamometer in this formula are as follows:

r, 4.9968 cm.; K, 2251.89 cm^2.-gm.; D, 48.108 cm.;
L, 41.621 cm.; N, 576; n, 45.

The period of vibration of the weighted phosphor-bronze wire was determined at several temperatures just before its use in the electrodynamometer and again immediately afterwards. The period was slightly longer than it was a year ago, but the temperature coefficient was unchanged for the new temperature curve, Fig. 2, is parallel to the old one. It is highly probable that this wire has suffered some oxidation during the year.

The dimensions of both coils were measured over again. The movable coil had to be rewound, as the flattened copper wire was loose. The new dimensions are appreciably smaller than those for a year ago. The rubber continues to shrink somewhat, and it is proposed to try new materials for the purpose hereafter. The periods T in the table are taken from the curve I, Fig. 2, at points denoted by the mean temperatures. Substituting the numerical values we have the following results for the cells at the reading of 15° on the thermometer:

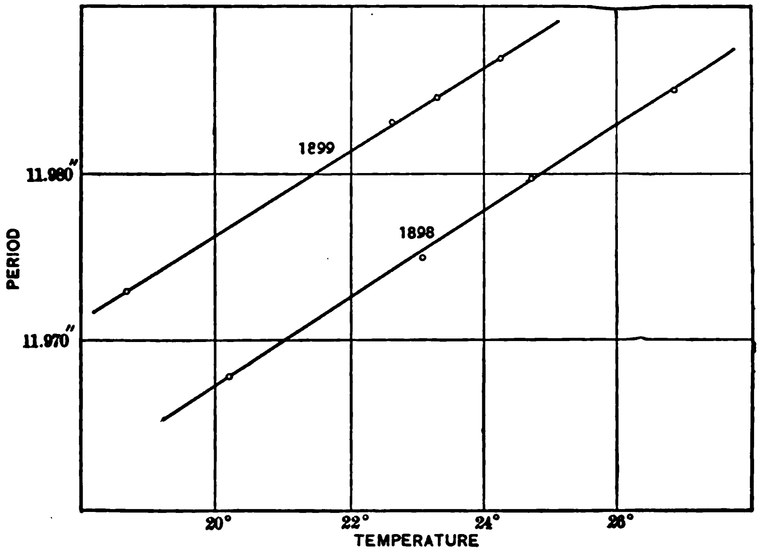

Fig. 2.

May 27, $E = 1.43355$ volts.
May 30, $E = 1.43333$ volts.
May 31, $E = 1.43305$ volts.

The dimensions of the movable coil were taken immediately after the last set of observations. Since it is known that its dimensions change by shrinking, and as a smaller value of r increases the value of the constant a, and hence the value of E, it is evident that any shrinkage between May 27 and May 31 tends to increase the value of E from the first set of observations. We have accordingly thought best to give the first determination half the weight of the other two. The result, after correcting for the thermometer error of 0°.05, is

1.4333 volts at 15° C.

If we allow equal weights for the three determinations, the result differs by only one unit in the fourth decimal place. Since the two cells themselves differ by one part in ten thousand, and since we do not claim a degree of accuracy greater than one part in five thousand, the fourth decimal place is of course in doubt.

In 1892 Glazebrook and Skinner made a very careful determination of the E. M. F. of the Clark cell by the silver voltameter method, assuming 0.001118 as the silver equivalent. The result was 1.4342 at 15°.[1]

Kahle in 1898 found by means of a Helmholtz electrodynamometer the value for the modified H-form, 1.4328 at 15°.[2] But Kahle says that the silver equivalent for a solution neutralized with silver oxide is five parts in ten thousand higher than for a solution not so neutralized. He obtained for the silver equivalent 0.0011182. But neither he nor Glazebrook apparently neutralized with silver oxide. Hence if we employ 0.0011187 for a solution not neutralized by silver oxide and recalculate from Glazebrook's data, the result is 1.4332 at 15°. This is practically identical with ours.

Physical Laboratory, University of Michigan, July 10, 1899.

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[1] Phil. Trans. (A), Vol. 183, p. 567, 1892.

[2] Zeitsch. f. Instrum. Kunde, June, 1898.

Note.—Since this paper was written the two Clark Cells used in the investigation have been taken by Professor Carhart to Berlin, and through the courtesy of the President of the Reichsanstalt, Professor Kohlrausch, of the Director of the technical department, Professor Hagen and of Professor Lindeck, the opportunity has been afforded of making several series of comparisons with the standards of the Reichsanstalt in Charlottenburg.

Unfortunately cell No. 2 was injured during the long journey, but No. 1 remained in good condition. No. 2 was often compared with No. 1 in Ann Arbor and was never more than one part in 10,000 lower than No. 1.

The result of three elaborate series of comparisons is that the E. M. F. of No. 1 is one part in 20,000 higher than the mean of all the normal Clark cells in both departments of the Reichsanstalt. This difference is well within the degree of accuracy attempted in this investigation.

√

THE RESISTANCE OFFERED BY IRON WIRES TO ALTERNATING CURRENTS.

BY ERNEST MERRITT.

THE abnormally large resistance offered by iron wires to alternating currents has been a matter of common observation for many years. The increased resistance is due to what is known as the "throttling effect," which prevents an alternating current from penetrating completely to the center of the wire; the effect is especially noticeable in iron because of the high permeability. The results of this throttling effect in the case of iron wires are sometimes quite startling, as for example in the case cited by Lord Kelvin, where the accidental short-circuiting of an alternating circuit by an iron bar caused the outer surface of the bar to be heated almost to redness, while the inner portions were scarcely warmed.

In the case of the non-magnetic metals, for which the permeability is constant, the computation of the resistance offered to alternating currents of a given frequency is a matter of no great difficulty. But in the case of iron the permeability changes with the current strength in accordance with a law that is as yet unknown. By assuming what appears to be a reasonable average value for μ and substituting this in the formula derived on the assumption of μ constant, we can obtain an approximate value for the resistance offered by an iron wire under given conditions; but, as I have pointed out in a previous article,[1] we have no means of telling how close this approximation will be. In fact it is questionable whether a formula derived upon the assumption of a constant permeability can be correct even in its general form; for it is to be remembered that the value of μ will not only change during the course of an alternation, but will also be different at a given instant in different parts of the wire, since the iron will be magnetized to a different extent at the surface from what it is at the center. The whole question is so complicated that an experimental study of the subject seems necessary before correct conclusions can be reached.

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[1] The Distribution of Alternating Currents in Cylindrical Wires, PHYSICAL REVIEW, Vol. 5, p. 47.

The experiments described below were carried out at my suggestion by Mr. H. H. Denio,[1] to whose care and patience the consistent results are due. The method employed is open to criticism for several reasons, so that no high degree of accuracy can be claimed for the numerical values obtained. Nevertheless I think that the results will serve a useful purpose in calling attention to the more important peculiarities in the behavior of iron wires, and that they may be of assistance in planning a more accurate study of the phenomena.

The iron wire whose resistance was to be measured was made one arm of a Wheatstone Bridge, the other arm being formed by large wires of German silver. The wires were not coiled, but stretched between two heavy brass blocks, *AB* and *CD* (Fig. 1), these being mounted on a long board. The distance between *A* and *C* was 220 cm. The resistance of the German silver wire *BD* was about 0.5 ohm. Current, either alternating or direct as desired, entered the bridge at *AB* and *CD* and divided between the two branches in the ordinary way. The balance of the bridge was determined by means of a telephone, *T*, one of whose terminals was permanently connected to one end of the iron wire *MC*, while the other could be moved along *BD* until the sound in the telephone reached a minimum. Complete silence was never obtained.

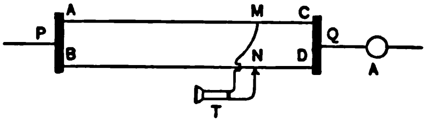

Fig. 1.

It was at first thought that the failure to obtain silence in the telephone was due to the fact that the inductance of *AC* was greater than that of *BD*. Several attempts were made to remove the difficulty by increasing the self induction of *BD*, but without success. I am now of the opinion that the trouble was caused by the variations occurring in the self induction of the iron wire during the course of an alternation. If this is true the difficulty is one that is inherent in the method.

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[1] Thesis for the degree of B. S., 1898. Cornell Universty Library.

Although complete silence could not be obtained, it was always possible to set to a minimum of sound in the telephone with considerable certainty. A change in the quality of the sound in passing through the minimum often assisted in locating *N*.

The alternating current used had a frequency of about 130 periods per second.

It was expected that the resistance offered to alternating currents would depend upon the current strength. The current flowing in the iron wire was therefore varied through a wide range of values, the resistance being measured for each. Curves were then plotted showing the relation between current and resistance for each wire tested. The resistance for direct current was determined by the same apparatus. Since the current used was often strong enough to appreciably heat the wire, the measurements made with direct currents were also repeated with a number of different current strengths. The ratio of the alternating current resistance to the direct current resistance for the same current strength is then a measure of the throttling effect. It will be observed that this method of procedure eliminates the errors that might arise from changes in resistance due to the heating of the wire.

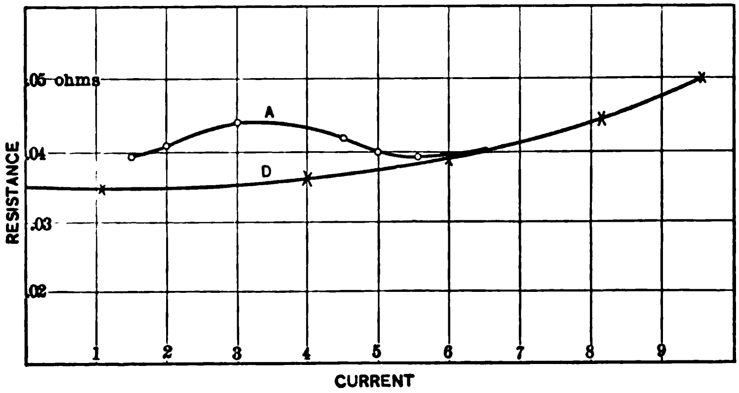

Fig. 2.

Seven different wires were tested, ranging in diameter from 0.032 cm. to 0.476 cm. The curves shown in Fig. 2, which refer to a wire of 0.114 cm. in diameter, are typical of the behavior observed in all cases. The lower curve (*D*) shows the variation in resistance with current strength as determined by using a direct current in the bridge. The increase in resistance in this case is due to the wire becoming heated. Curve *A* shows the resistance offered to alternating currents, also plotted as a function of the current strength. In Fig. 3 a similar set of curves is shown for a wire 0.366 cm. in diameter. With small current it was difficult to measure the resistance with accuracy, so that the portion of the curves near the origin are less reliable than those corresponding to larger currents.

In their general character the results represented by these curves are what might be anticipated from the known magnetic properties of iron. For small currents the iron is only weakly magnetized, so that its permeability is small. For such currents the behavior of the iron is therefore not very different from that of the non-magnetic metals, whose resistance, for the frequency and sizes of wire used, should be practically the same for alternating currents as for direct currents. As the current in the wire is increased the resulting magnetization becomes greater, and for a time the permeability rapidly increases. During the range of current values for which μ is large the throttling effect is well marked, the alternating current resistance being appreciably larger. The increase in resistance is of course greater for large wires than for small ones. If the current is increased still further, the outer layers of the wire begin to satu-

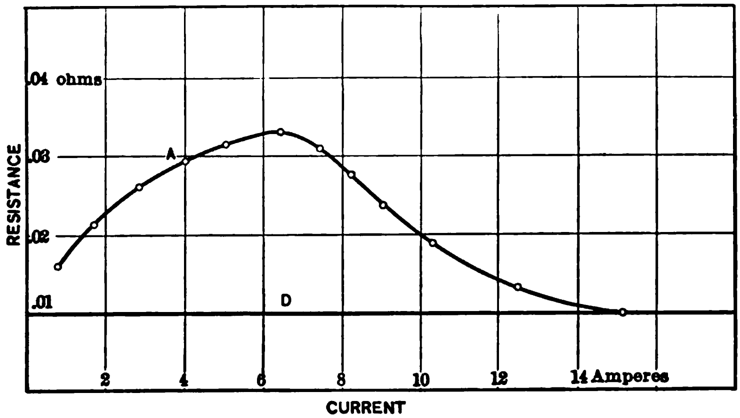

Fig. 3.

rate, and the permeability of this part of the iron is in consequence diminished. Roughly speaking, the wire is now equivalent to a core of high permeability, which is surrounded by a shell of practically non-magnetic material. The thickness of this saturated or non-magnetic layer increases with increasing current until finally the wire is saturated throughout, after which it again behaves like a non-magnetic substance and the resistance for alternating currents becomes appreciably the same as that for direct currents. The current for which the alternating current resistance is a maximum, as well as the relative value of this maximum resistance, will evidently depend upon the size and magnetic qualities of the wire.

A glance at the curves plotted in Figs. 2 and 3 shows that the results obtained by Mr. Denio are qualitatively the same as the above considerations would lead us to expect. Any quantitative test of these conclusions must of necessity be very incomplete. It is gratifying to observe, however, that the measured values of the alternating current resistance do not differ greatly from those resulting from an approximate theoretical treatment. In the case of the smaller wire, whose diameter was 0.114 cm., an alternating current of 3.5 ampères, corresponding to a maximum of about 5 ampères, would give $H = 18$ as the maximum magnetizing force at the surface. The average permeability of soft iron when carried through a cycle having this maximum magnetizing force is not far from 1000.[1] For the interior of the wire the value of H would be less than 18 and permeability would therefore be somewhat larger. If we put the average permeability of the wire equal to 1600, the specific resistance being about 10,000, the ratio of the alternating current resistance to the ordinary resistance of the wire should be about 1.15. The observed value of this ratio, as shown by the curves in Fig. 2, was 1.34. The discrepancy between these two values, although considerable, is probably not greater than should be expected, for the computed value depends on the assumption of a *constant* permeability. For a current of 3.5 ampères the outer layers of the wire are already approaching saturation. For still larger currents the permeability would therefore be less than 1600, and we should expect the alternating current resistance to diminish with increasing current. That this conclusion is verified by experiment is shown by the curve in Fig. 2.

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[1] Ewing, Magnetic Induction in Iron and other Metals, p. 107.

A similar computation in the case of the larger wire (diameter = 0.366 cm.) shows that for a current of 7 ampères the alternating current resistance should exceed that for direct currents in the ratio of about 2.8 to 1. The value of this ratio determined by experiment was 3.2. But for a current of 7 ampères the magnetic force at the surface is only slightly greater than unity, so that saturation is not yet reached. The rapid decrease in resistance shown in Fig. 3 for larger currents is therefore hard to explain.

A number of other wires were tested, the results being in general more satisfactory with the wires of larger diameter. While the cases cited above are typical, it must be admitted that in some instances the quantitative agreement between observed and predicted results was even less satisfactory than in the case mentioned. Further experiments with more reliable methods will doubtless indicate more clearly in what way the approximate theory must be modified in order to become applicable to the magnetic metals.

ON A THEOREM OF CLEBSCH'S.

By Paul Saurel.

CLEBSCH has shown[1] that the integrals of the equations of motion of a vibrating elastic solid can be put into a very simple and elegant form. His demonstration, while by no means long or difficult, seems, nevertheless, to admit of some simplification.

The equations in question are :[2]

$$\left.\begin{aligned} \frac{\partial^2 u}{\partial t^2} &= a^2 \varDelta u + (b^2 - a^2)\frac{\partial \sigma}{\partial x} \\ \frac{d^2 v}{\partial t^2} &= a^2 \varDelta v + (b^2 - a^2)\frac{\partial \sigma}{\partial y} \\ \frac{\partial^2 w}{\partial t^2} &= a^2 \varDelta w + (b^2 - a^2)\frac{\partial \sigma}{\partial z} \end{aligned}\right\} , \qquad 1$$

in which
$$\varDelta = \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}$$

and
$$\sigma = \frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} \, .$$

From the above equations, it follows without difficulty that

$$\frac{\partial^2 \sigma}{\partial t^2} = b^2 \varDelta \sigma. \qquad 2$$

Let u, v, w be any given solution of the above equations and let us write

$$\left.\begin{aligned} u &= \frac{\partial P}{\partial x} + u' \\ v &= \frac{\partial P}{\partial y} + v' \\ w &+ \frac{\partial P}{\partial z} + w' \end{aligned}\right\} . \qquad 3$$

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[1] Clebsch, Ueber die Reflexion an einer Kugelfläche. Borchardt's Journal für die reine u. angew Math., Bd. 61.

[2] Kirchhoff, Vorlesungen über Mechanik, XI Vorlesung.

From these we obtain

$$\sigma = \Delta P + \sigma' \qquad 4$$

in which
$$\sigma' = \frac{\partial u'}{\partial x} + \frac{\partial v'}{\partial y} + \frac{\partial w'}{\partial z}\,.$$

If we substitute the above values of u, v, w in the equations of motion, we get

$$\left.\begin{aligned}
\frac{\partial^2 u'}{\partial t^2} + \frac{\partial}{\partial x}\left[\frac{\partial^2 P}{\partial t^2} - b^2\Delta P\right] &= a^2\Delta u' + (b^2 - a^2)\frac{\partial \sigma'}{\partial x}\\
\frac{\partial^2 v'}{\partial t^2} + \frac{\partial}{\partial y}\left[\frac{\partial^2 P}{\partial t^2} - b^2\Delta P\right] &= a^2\Delta v' + (b^2 - a^2)\frac{\partial \sigma'}{\partial y}\\
\frac{\partial^2 w'}{\partial t^2} + \frac{\partial}{\partial z}\left[\frac{\partial^2 P}{\partial t^2} - b^2\Delta P\right] &= a^2\Delta w' + (b^2 - a^2)\frac{\partial \sigma'}{\partial z}
\end{aligned}\right\} \qquad 5$$

Suppose that
$$\sigma' = 0. \qquad 6$$

The necessary and sufficient condition for this, is (equation 4) that

$$\Delta P = \sigma. \qquad 7$$

Suppose moreover that P satisfies the equation

$$\frac{\partial^2 P}{\partial t^2} = b^2\Delta P. \qquad 8$$

Equations 5 then become

$$\left.\begin{aligned}
\frac{\partial^2 u'}{\partial t^2} &= a^2\Delta u'\\
\frac{\partial^2 v'}{\partial t^2} &= a^2\Delta v'\\
\frac{\partial^2 w'}{\partial t^2} &= a^2\Delta w'
\end{aligned}\right\} \qquad 9$$

If, then, it be possible to choose P so that equations 7 and 8 are simultaneously verified, equations 3 will determine u', v', w'. We shall thus have established that every solution of the given equations can be written in the form of equations 3, P being a common integral of equations 7 and 8, and u', v' w' being three solutions of the equation

$$\frac{\partial^2 \varphi}{\partial t^2} = a^2\Delta\varphi, \qquad 10$$

these solutions, moreover, being connected by equation 6:

$$\frac{\partial u'}{\partial x} + \frac{\partial v'}{\partial y} + \frac{\partial w'}{\partial z} = 0. \qquad 11$$

In order to show that we can determine P so as to satisfy equations 7 and 8, let us write the equation obtained from them:

$$\frac{\partial^2 P}{\partial t^2} = b^2\sigma. \qquad 12$$

The pair of equations 7 and 8 may be replaced by the pair of equivalent equations 7 and 12. The integral of equation 12 is

$$P = b^2 \int dt \int \sigma dt + A(x, y, z) \cdot t + B(x, y, z),$$

A and B being two arbitrary functions. From this equation we obtain

$$\Delta P = b^2 \int dt \int \Delta\sigma dt + \Delta A(x, y, z) \cdot t + \Delta B(z, y, z).$$

Choosing the arbitrary functions so that

$$\Delta A(x, y, z) = 0, \quad \Delta B(x, y, z) = 0$$

and making use of equation 2, we obtain

$$\Delta P = \int dt \int \frac{\partial^2 \sigma}{\partial t^2} dt = \sigma.$$

Thus equation 7 is also verified and the theorem stated above is established.

This result may be put into a more striking form if we make use of a theorem tacitly admitted by Clebsch and rigorously established by Duhem.[1] The theorem in question is the following: If u', v', w' are three integrals of equation 10, which also verify equation 11, we may write

$$u' = \frac{\partial W}{\partial y} - \frac{\partial V}{\partial z},$$
$$v' = \frac{\partial U}{\partial z} - \frac{\partial W}{\partial x},$$
$$w' = \frac{\partial V}{\partial x} - \frac{\partial U}{\partial y},$$

in which U, V, W are three solutions of equation 10.

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[1] Duhem, Sur l'intégrale des équations des petits mouvements d'un solide isotrope. Mémoires de la Société des Sciences de Bordeaux. 5^e Série, t. III., p. 325, 1899.

Thus finally we obtain Clebsch's theorem: Every solution of the equations of motion of a vibrating elastic solid can be written in the form:

$$\left.\begin{aligned} u &= \frac{\partial P}{\partial x} + \frac{\partial W}{\partial y} - \frac{\partial V}{\partial z} \\ v &= \frac{\partial P}{\partial y} + \frac{\partial U}{\partial z} - \frac{\partial W}{\partial x} \\ w &= \frac{\partial P}{\partial z} + \frac{\partial V}{\partial x} - \frac{\partial U}{\partial y} \end{aligned}\right\}, \qquad 13$$

U, V, W being three solutions of the equation

$$\frac{\partial^2 \varphi}{\partial t^2} = a^2 \Delta \varphi \qquad 14$$

and P being a solution common to the equations

$$\begin{aligned} \frac{\partial^2 P}{\partial t^2} &= b^2 \Delta P \qquad 15 \\ \Delta P &= o. \end{aligned}$$

Conversely, if U, V, W are any three solutions of equation 14, and P any solution of equation 15, equations 13 furnish a solution of the equations of motion. This is very easily verified by substituting in the original equations the values of u, v, w given by equations 13.

BORDEAUX, April, 1899.

AN INVESTIGATION OF THE MAGNETIC QUALITIES OF BUILDING BRICK.

O. A. GAGE AND H. E. LAWRENCE.

THE magnetic disturbances existing in physical laboratories have in a few cases, been traced to the brick work of the walls and piers of the building. Having in mind the selection of a brick suitable for a laboratory of physics, it became desirable to investigate, so far as possible, the causes and behavior of the magnetism known to exist in some building bricks. For this purpose samples were furnished the writer from many of the factories of the northern and eastern states. These included all varieties of shades of color of which an attempt is made to convey an idea to the reader in the accompanying tables. The greater number were of the pressed brick variety, though specimens of the ordinary red variety are included. In all the samples but one—number 24, nearly white in color—magnetism was found to a greater or less degree.

The apparatus used was a magnetometer of the ordinary "end-on" form, constructed for the purpose, the telescope being mounted at a distance of about 125 cm., the center of the brick when in position being about 14 cm. from the magnetometer needle. The position of the poles was found approximately by presenting the different faces in turn to the magnetometer; the two opposite causing the greatest deflection were said to contain the poles. In all cases these were found to be either in the faces or edges, never in the ends of the bricks. This position was permanent and could not be changed by any means at hand.

The opposite table gives the results first obtained on thirty-one samples.

Although a large number of the same kind was not examined, yet in a few cases duplicates are included. Nos. 5, 9, 10, 11, 12 are alike and their moments, 0.330, 0.256, 0.256, 0.282, 0.209, re-

Brick.	Color.	Deflection.	Angle of Deflection.	Moment.	Position of Poles.
1	Ordinary Red.	0.9	17′14″	0.924	Edge.
2	" "	0.55	10 32	0.565	"
3	" "	0.575	11 1	0.591	"
4	" "	0.475	9 6	0.495	"
5	Pressed "	0.3	5 45	0.330	"
6	Cream.	0.5	9 35	0.558	"
7	Brown.	2.25	43 5	2.476	"
8	Pressed Red.	0.55	10 32	0.573	"
9	" "	0.25	4 47	0.256	"
10	" "	0.25	4 47	0.256	"
11	" "	0.3	5 45	0.282	"
12	" "	0.2	3 50	0.209	"
13	Brown.	1.0	19 9	1.116	"
14	"	1.05	20 7	1.172	"
15	"	1.05	20 7	0.821	Face.
16	Brown Spotted.	0.35	6 42	0.362	Edge.
17	" "	0.2	3 50	0.206	"
18	" "	0.3	5 45	0.308	"
19	Cream.	0.15	2 52	0.160	"
20	"	0.3	5 45	0.326	"
21	Red Spotted.	0.3	5 45	0.326	"
22	" "	0.1	1 55	0.078	Face.
23	Cream Spotted.	0.1	1 55	0.109	Edge.
24	White.	0.000		0.000	
25	Pressed Red.	3.0	57 26	2.428	Face.
26	" "	2.65	50 44	2.070	"
27	" "	2.55	48 50	1.992	"
28	White.	0.05	0 53	0.035	"
29	"	0.05	0 53	0.035	"
30	"	0.05	0 53	0.049	Edge.
31	"	0.05	0 53	0.049	"
32	"	1.00	19 9	0.997	"

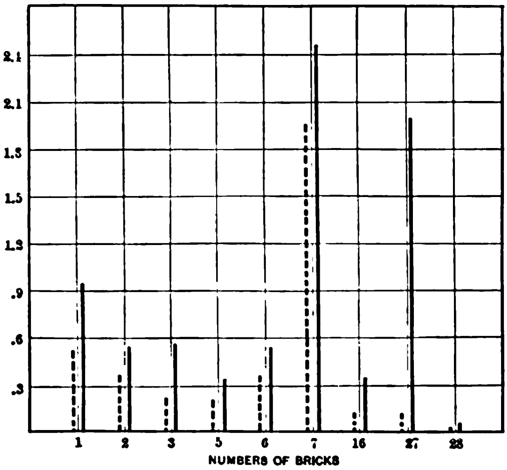
spectively, show no great variation. The bricks in each of the following series are alike: Nos. 13, 14, 15; Nos. 16, 17, 18; Nos. 25, 26, 27; Nos. 24, 28, 29, 30, 31; the moments of those belonging to the some set being fairly equal in view of the variable composition of the brick.

The moments of the bricks are not constant. The foregoing readings were taken in October, 1898. The experiments were resumed in January, 1899. The following table gives the measurements taken then:

Brick.	Deflection.	Moment.	
		Oct., '98.	Jan., '99.
2.	0.55	0.565	0.565
3.	0.5	0.591	0.513
5.	0.7	0.330	0.770
6.	1.4	0.558	1.562
7.	14.62	2.476	15.994
8.	0.4	0.573	0.416
16.	0.1	0.362	0.103
20.	0.2	0.326	0.217
21.	0.1	0 326	0.107
23.	0.8	0.109	0.868
25.	3.0	2.428	2.428
27.	2.3	1.992	1.797
28.	0.1	0.035	0.076

It will be noticed that the bricks did not vary in the same direction, some increasing, others decreasing in strength.

No good idea of the strength of the brick can be obtained from the value of its moment; but when compared with a steel magnet, its force is better apprehended. Assuming that the steel magnet has 0.325 c.-g. s. units to the cubic millimeter, brick No. 7 at its first strength would be equivalent to a steel magnet 9.7 mm. in length and 1 mm. in diameter; at its new strength, to a magnet 62.7



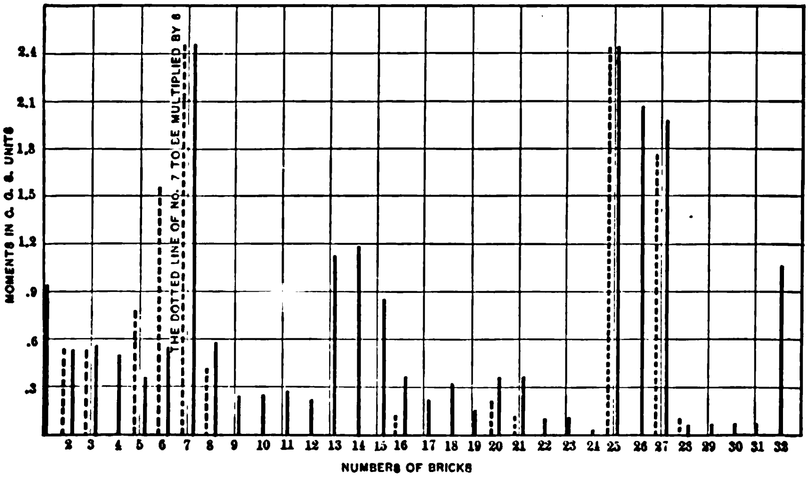
Black lines show moments at ordinary temperature; dotted lines show the same when heated.

mm. in length and 1 mm. in diameter. Brick No. 1 would equal a similar magnet 3.53 mm. in length.

The moments of the bricks were changed when rapped in the field of a motor and also when heated. The temperature to which they were heated was low; but, nevertheless, the diminution of the moments was in all cases marked, and, in some, extraordinary. In all cases the bricks regained their strength upon cooling. The results obtained are tabulated and shown below.

Brick.	Deflection.	Temperature.	Moment.		Ratio.	
			Hot.	Cold.	Cool.	Hot.
1.	0.55	110°C.	0.56	0.92	1	0.61
2.	0.35	130	0.36	0.56	1	0.64
3.	0.25	130	0.26	0.59	1	0.44
5.	0.2	140	0.22	0.33	1	0.67
6.	0.325	110	0.36	0.56	1	0.64
7.	1.8	130	1.98	2.48	1	0.80
16.	0.15	140	0.15	0.36	1	0.42
27.	0.15	130	0.12	1.99	1	0.06
28.	0.00	130	0.00	0.04		

An interesting point in this variation is the ratio at which the various bricks diminish in strength. If the bricks containing the same clay had been used, the constancy of the ratio would not be



Black lines show moments in October, 1895; dotted lines show the same in January, 1899.

surprising; but when bricks, quite different in composition, lose their magnetism so uniformly, it is certainly strange. Omitting bricks Nos. 27 and 28, the average of the ratio is 1 : 0.6 and four of these differ very slightly from the mean.

The bricks were also tested for permeability. The magnetometer method was used and in the first coil the wire was wound perpendicular to the long axis of the brick and consisted of 185 turns. Accordingly, the magnetic axes of the brick and the coil were at right angles. In a second coil of 125 turns, the magnetic axis was parallel to that of the brick. The coils were so wound that the brick could be removed, and its permeability was tested by noting whether its presence or absence produced any effect upon the magnetometer needle. Currents as large as four ampères were used, yet the bricks apparently had no effect upon the strength of the coils.

No experiments were performed to prove directly the cause of the magnetism; but, in the course of some previous ones, light was thrown upon the subject. The greater part of the magnetism is probably due to the presence of the magnetic iron oxide, for the powder from several of the bricks gave up small particles to the magnet, and iron is the only constituent of the clays that would act this way. Also, the larger amount of iron oxide in a clay makes the brick more magnetic, other things being equal. Thus brick No. 25 contains about five times as much iron as No. 28 and is very much stronger. Brick No. 6 contains about the same amount of iron as No. 25, but is not nearly so strong. This may be due to the fact that No. 6 has over 14% of lime and 4% of magnesium, while No. 25 has no lime and only 0.5% of magnesium. The iron is supposed to form double silicates of lime and iron similar to the mineral zoisite, and, in this form, the iron is probably non-magnetic. Nos. 1 and 3 are also stronger than 6 when they contain less iron and also less lime and magnesium.

The permanent magnetism is probably due to the cooling of the bricks in the magnetic field of the earth. One or two facts seem to support this theory. The position of the poles was somewhat remarkable in that it was never found in the head of the brick. This may be explained by the way in which they are placed in the kilns to be burned. They are placed always on the face or edge allowing the magnetic lines of the earth to pass through either both faces or both edges. No direct proof of this was obtained, but one brick similar to No. 1 of the ordinary red variety, which had not been burned, was tested and contained no magnetism. Accordingly, it may be that burning is the agent which gives the bricks their permanent magnetism.

To recapitulate, the bricks were, in most cases, found to be permanent magnets of slight and varying strength. The magnetism was affected by heat and strong magnetic fields. The probable cause is the presence of magnetic iron oxide, either a constituent of the clay or formed by heat.

NOTE.

/ *Robert Wilhelm Bunsen.*—By the death of Professor Bunsen, who passed away at a ripe old age, in Heidelberg, on the 15th of August, chemistry and physics lose one of the most gifted men who ever devoted his life to their advancement. It would indeed be difficult to say which of these two sciences owes most to him.

Bunsen was born in Göttingen on March 25, 1811, where his father was professor of theology. Born and brought up in academic circles, he completed his schooling early. He received his doctor's degree in the University of his native city in 1830 and it was in that institution, in 1833, that he began his career as privatdocent. In 1836 he was appointed to the professorship of chemistry in the polytechnic school at Cassel, and in 1838 to a similar chair in the University of Marburg, which position he filled for thirteen years. In 1851 he was appointed professor of chemistry at Breslau and in the following year accepted the chair in that science at Heidelberg, which he filled up to the time of his retirement, after more than forty years of active service.

In reviewing Bunsen's work, one is struck with the importance of much of it, considered from a strictly utilitarian point of view. His was, however, a utilitarianism which never developed into commercialism. No man was ever a more staunch adherent to the cause of pure science than Bunsen, and while he turned out one useful device after another, with a fertility and ingenuity which would have made his fortune, had he turned his attention to industrial pursuits, he showed not the slightest tendency to forsake the high calling of a scientific investigator for that of the inventor. So far, indeed, was his nature free from worldliness that he was one of the few eminent men of science in all Germany who, during the rush to the great centers during the early years of the empire, was not to be coaxed from his quiet life in a small university town. In Heidelberg, the scene of his most important scientific achievements, he died as he had lived, a simple German University professor of the old school.

The first piece of work published by Bunsen after his graduation from the university, is characteristic of the intense interest which he always had in the practical as well as the purely scientific aspects of a subject. It dealt with the remarkable properties of freshly prepared ferric hydroxid as an antidote for arsenical poisoning. It was a discovery of genuine importance, for this material has since been the standard remedy in such cases, and has been the means of saving many lives. In the following year, 1835, he began his well-known experiments upon the double cyanides, and in 1837 he laid the foundation of an important branch of chemistry, that of the organic compound radicals. His discovery of cacodyl (C_2H_6As) and his investigations of the remarkable properties of its highly poisonous, inflammable and explosive compounds, forms one of the most fascinating chapters in the history of science. Roscoe in his account of the early work of Bunsen, written for *Nature* in 1881, compared his experience in this exploration to that of a traveler "in an unknown and treacherous land, without sign posts to guide him, or more assistance on his travels than was furnished by his own scientific acumen and his unfaltering determination." In the course of this work he had one eye blinded by an explosion, and very nearly lost his life by breathing the poisonous vapors.

Bunsen's memoir upon this subject was published in 1837, and he almost immediately turned his attention to the investigation of the gases given off by blast furnaces, in the course of which he was led to the development of the methods of gas analysis, which were brought together many years later, in his famous treatise entitled *Gasometrische Methoden.* The methods of manipulation employed in this work are those of the physicist, and it is probably due to his experience with them that Bunsen was turned from the domain of pure chemistry to the middle ground of chemical physics, in which he was one of the chief investigators of his time.

It is with his contributions to the science of physics that we have to do in this notice, and it is possible in considering simply this phase of his scientific activity to do little more than to mention the succession of important contributions which we owe to him. In 1841 he invented the Bunsen battery, in which carbon was substituted for the platinum as the negative pole of the voltaic cell. From 1843, when his last paper on this subject appeared, until 1847, was a period of quiescence. In the latter year he visited Iceland, and one of the results of this visit was his theory of the geyser, together with the classical experiments by means of which he imitated, artificially, its action and verified his views of the phenomena. These studies were followed in 1854 by the production of chromium and other metals by electrolysis. In 1857, with the colaboration of Roscoe, he completed his remarkable photochemical studies; in the course of which the action of light in bringing about the union of hydrogen and chlorine, was exhaustively investigated.

About 1860 Bunson formed a scientific partnership with his friend and colleague, Gustav Kirchoff, then professor of physics in Heidelberg, which was to result in the most brilliant achievement of his own scientific career, probably of the decade. This was the application of the study of the spectrum to chemical analysis, and the extension of such analysis by the identification of the black lines in the solar spectrum with the bright lines found in the spectra of metals, to solar and stellar atmospheres. One of the fruits of this investigation was the discovery of cæsium and rubidium by means of their spectra, a performance which, taken by itself, would have sufficed to have stamped the work as of the highest importance to both physicists and chemists.

The last really important work published by Bunsen is contained in his paper entitled "Calometrische Untersuchungen" (*Poggendorf's Annalen*, vol. 14, 1870). It was in this paper that his well-known ice calorimeter is described, an instrument which, in the hands of its inventor, gave results of a degree of delicacy hitherto unattained in calorimetric work, and made it possible to determine with accuracy the specific heats of materials of which only very small quantities were available. For several years, indeed, he continued to contribute to the *Annalen*, publishing several papers on spectrum analysis, and exhibiting in 1883 a quality which had always characterized his scientific work, that of attention to minute detail, in his study on the progressive condensation of carbonic acid on glass surfaces. In the heated discussion which followed upon this subject between himself and Kayser, Bunsen showed that he was still able to defend his views vigorously, and to verify his conclusions by experimental work of the most delicate and trying character. His final contribution, a paper on the steam calorimeter, was published in the *Annalen* in the year 1887.

For several years after his scientific career, lasting nearly forty years, was ended, Bunsen, continued to be a figure on the streets of Heidel berg. Gradually, as old age came upon him, he retired more and more from the public life of the university town, where for half a century he had been the most notable character, and finally, in his eighty-ninth year, passed peacefully away.

Bunsen was no less eminent as a teacher than as an investigator. A generation of chemists, not only in Germany, but in England and in America, and many students in physics likewise, look back with gratitude to the days which they spent in his laboratory. His domain was a middle ground between the two sciences. A chemist by training, his greatest contributions to that science arose from his application to one field of research after another, of the methods of physics.

Of the numerous appliances which our laboratories owe to his ingenuity, the Bunsen burner, the Bunsen filtering pump, the Bunsen spectroscope, the Bunsen photometer and the Bunsen calorimeter, are perhaps the best known. These remain to remind us that Bunsen was not only a chemist and physicist, but he had in him the essentials of a successful inventor. Science owes him a debt of gratitude in that he did not yield to the temptation to enter upon purely an industrial field, but gave himself, from the beginning of his life to the end of it, solely to her service.

E. L. N.

NEW BOOKS.

A Text-book of General Physics. By Charles S. Hastings and Frederick E. Beach. Pp. viii + 768. Boston, Ginn & Company, 1899.

The evolution of the modern text-book of physics has been a rapid one. Thirty years ago we were still under the influence of the age of the discoverer and pioneer, the age of Davy and Faraday and Henry. Natural philosophy, to use the good old name, was still a wonderland to be exploited, and its attitude toward nature was still mainly descriptive. Phenomena were the principal thing, and the theoretical thread connecting them, in the elementary text-book, was but slender. For the general student, the great structures of modern physical theory still lay concealed in the mathematical formulæ of their first expositors. The order of treatment, as was natural, was historical, rather than logically progressive.

The introduction of laboratory methods, the spread of mathematical physics, the growth of the elective system in colleges, and the rise of the graduate schools made demands on elementary instruction that the text-books of the day could not supply. The laboratory demanded quantitative statements of relations, which could be tested by experiment. The advanced courses required a logical introduction, a firm grasp of definitions, a compendium, in short, of the elements on which physical theory is founded.

Thus mechanics, which thirty years ago was by many teachers of physics hardly touched upon in the college course, now underlies everything, and the aim even in the elementary text-book has become, not the description of phenomena, but the application of mechanical principles in all branches of the science. Meanwhile the enormous growth of the whole subject makes it necessary to narrow still more the range of phenomena described, so that elementary physics, in many modern text-books, has come to be treated entirely as a discipline, the chief aim of which is to train for the more advanced courses.

The progress in the treatment of the subject has been great, yet not without loss. The text-books of the day have become abstract, general and, in some cases, to the ordinary student, dry. The deductive side of physics has crowded out the experimental and the phenomenal, which really are the foundations. These remain, of course, in the laboratory, but the subjects which can be there introduced are few and simple.

For these reasons a book like the present one, which has to some extent the characteristics of the older type, or represents, as perhaps we should rather say a return of the pendulum which had swung too far, is to be welcomed. It is however not reactionary, but distinctly modern in its attitude. Theoretical discussion holds the first place, and is in fact in some cases carried to such detail as to be of questionable value in a general text-book. Descriptions of apparatus and phenomena are introduced chiefly for the illustration and application of theoretical principles. Mechanics is very fully treated, as the authors themselves remark, as the foundation of the whole science, and fully illustrated.

The discussion of circular motion affords an excellent illustration of the method of the book.

After an extended and rather fresh discussion leading to the usual equations, no less than fifteen different applications of the principle are taken up in detail including the conical pendulum, both in its simple form, and that more commonly used in chronographs and driving clocks, such machines as the centrifugual dryer and the cream separator, the gyroscope in two forms, and a variety of meteorological and astronomical problems depending on the rotation of the earth. Not only is the application of the principle in various ways thus clearly shown, but its importance in Nature is emphasized. The use of similar methods throughout the book leads to long discussions on special topics. These are always topics which by their intrinsic interest, and their wide applications to natural phenomena offer themselves easily to such treatment. Thus surface tension, the theory of solutions, the kinetic theory of gases, thermodynamics, wave-motion, are treated at unusual length and in a highly interesting manner.

The treatment of acoustics, following the chapter on wave-motion, is excellent, especially in its relations to music and musical instruments.

The chapters on electricity are less satisfactory. Many fundamental propositions are assumed without proof (as potential at a point, magnetic induction, hysteresis, the equivalence of a circuit and a magnetic shell) and so much ground is covered, so many subjects briefly touched, that the general impression is of confusion and overcrowding of material, and lack of assimilation of the parts. Some discussions would be practically unintelligible to a student from lack of previous definition of terms, as, for example, the introduction of "Faraday tubes" on page 510.

The subject of optics, on the other hand, is admirably handled, the portion on optical instruments especially so. The "wave-front" method of discussion is adopted from the beginning and is applied to the ordinary problems of reflection and refraction in a concise and elegant manner. There is a welcome chapter on the optical phenomena of the atmosphere, a subject too frequently ignored in text-books. The section on color-vision is clear and careful, though it must be said that the Young-Helmholtz theory cannot be trusted even to the extent to which the authors carry it to explain the phenomena of color-vision, and the insufficiencies pointed out by them are by no means the only ones.

The method of the book requires space, and the amount of material is considerably greater than in most of the more modern class-books, though not approaching the encyclopædic dimensions of some of the older treatises. The illustrations in the text are supplemented by a large number of well-selected problems at the end of each chapter.

The mechanical execution of the work is excellent, and but few slips, either of printing or in statement, have been noted. The book is a valuable addition to the college text-books now available.

FRANK P. WHITMAN.

Practical Physics. By WILLIAM WATSON. pp. xvi+238. London and New York, Longmans, Green & Co., 1896.

This useful little laboratory manual contains one hundred and ninety-three exercises for experiments in elementary physics. These are nearly all quantitative in character, and are designed to be carried out with the use of simple forms of apparatus. There are thirty-one experiments in simple mensuration, including the measurements of lines and of surfaces and of mass, volume and density. Hydrostatics and exercises upon the properties of gases precede the work upon mechanics of solids, which is an unusual arrangement of subjects. A short but very good section upon the geometrical representation of varying quantities deals with the use of cross section paper in the plotting of curves. The selection of material is almost always excellent.

The relation of the metric system to British measures is dealt with at the beginning of the volume and thereafter the metric system is used with a fair degree of consistency throughout. It is to be noted, however, as in many works written by those who have been brought up to the use of British measures, that these are employed in many instances in the description of apparatus. An example of this occurs in Exercise 78, entitled "Motion on an Inclined Plane," which reads as follows: "*Apparatus : Smooth board, about 6 feet long, and 6 or 8 inches wide; a long wedge about 3 inches thick at the wide end; . . . . Place the wedge under one end of the board so that this end is about one inch higher than the other. At about two inches from the higher end, draw a line across the board at right angles to the length, etc.*" In making measurements with this apparatus, however, centimeters are again employed. It is obvious that the author, like many other working physicists, still does his *thinking* in terms of the British units and that the use of the metric system is an artificial matter with him. It will continue to be so with students and teachers unless the writers of manuals and the directors of laboratories make it a practice to deal in the metric system to the rigorous exclusion of all others.

E. L. N.

Harper's Scientific Memoirs: The Laws of Gases. Memoir by ROBERT BOYLE and E. H. AMAGAT. Translated and edited by CARL BARUS.

The Second Law of Thermodynamics. Memoirs by CARNOT, CLAUSIUS and THOMSON. Translated and edited by W. F. MAGIE.

The Fundamental Laws of Electrolytic Conduction. Memoirs by HITTORF and F. KOHLRAUSCH. Translated and edited by H. M. GOODWIN. New York, 1899.

No more fortunate selection could have been made for the fourth volume of this series than the translation of Amagat's celebrated papers on the *Compressibility of Gases at High Pressure* for as the translator, Professor Carl Barus, says in his terse but pointed preface, everybody cannot so easily get these papers into his possession.

Amagat's memoirs belong to the class of scientific literature which one would like to have at his elbow, and the scientific men of America are indebted to Professor Barus for making this possible.

One cannot say as much for the materials contained in the volume on the "Second Law of Thermodynamics." Since an excellent American translation of Sadi Carnot's *Memoir on the Motive Power of Heat* has recently been published, and since Kelvin's paper on the *Dynamical Theory* has likewise been rescued from its hiding place in the transactions of the Royal Society of Edinburgh, and has been incorporated in volume I. of his *Mathematical and Physical Papers*, there would seem to be little occasion for the reprinting of either. As to the celebrated paper by Clausius, which is inserted between the translation of Carnot, and the reprint of Kelvin in this volume, that likewise can be regarded as reasonably accessible to the student of physics. It appeared in the Berlin Academy, February, 1850, was reprinted in Poggendorff's Annalen of that year, volume 79, was translated in the *Philosophical Magazine*, fourth series, volume II., subsequently formed the first chapter of *Clausius' Theory of Heat*, 1864, was for the second time converted into English in the translation of that work by Hirst in 1867, and in 1898 it reappeared, with notes by Max Planck as volume 99 of the *Ostwald's Klassiker*. No one questions the interest and importance of this paper, nor of the others which have been gathered together with it to form this volume of the scientific memoirs. It is unquestionably a useful thing to gather three related treatises from different sources together within the same covers. Still less would any critic be inclined to object to anything which might lead to a wider dissemination of such literature; but are there not very many papers as important and as closely related to one another as these, which are inaccessible, and which might with still greater service to the scientific public be grouped together?

The third of the reprints under consideration is not open to the above objections. In culling from *Faraday's Experimental Researches* those sections which deal with electrolysis, and arranging them so as to form a straightforward and consistent treatise, Professor Goodwin has done the scientific public a real service. Faraday's paper on "Electrochemical Decomposition" forms a fitting introduction to the volume, and the translation of Hittorf's papers, which in spite of their great significance are less widely known than they ought to be, forms a fitting appendix to Faraday's work. The volume ends with a translation of Kohlrausch's brief paper on "Conductivity of Electrolytes Dissolved in Water," in relation to the migration of their components.

E. L. N.

INDEX TO VOLUME IX.

A.

Acetylene and hydrogen, photometric study of mixtures of, burned in air, *Hartman*, 176.

Alternating currents, resistance offered by iron wires to, *Merritt*, 294.

Amalgams, lead, the electrical resistance of, at low temperatures, *Gressman*, 20.

American association for the advancement of science, 254.

Atwater, W. O., A new respiration calorimeter, and experiments on the conservation of energy in the human body, 129, 214.

B.

Barus, Carl, *The Laws of Gases*, 316.

Beach, Frederick E., *Text-book of General Physics*, 313.

Boiling and freezing water simultaneously, *Quick*, 121.

Book Reviews.
- An introduction to the mathematical theory of attraction, *Tarleton*, 191.
- Harper's scientific memoirs: Vol. III., Röntgen rays; Vol. IV., Modern theory of solutions, 64; The Laws of Gases, The Second Law of Thermodynamics, The Fundamental Laws of Electrolytic Conduction, 316.
- Matter, energy, force and work, *Holman*, 59.
- Ostwald's Klassiker der exakten Wissenschaften, 127.
- Physical problems and their solutions, *Bourgougnon*, 127.
- Practical physics, *Watson*, 315.
- Radiation, an elementary treatise on electromagnetic radiation and on Röntgen and cathode rays, *Hyndman*, 252.
- Scientific papers, *Tait*, 123.
- Text-book of general physics, *Hastings and Beach*, 313.
- Stars and telescopes, *Todd*, 192.

Brick, on the magnetic qualities of, *Gage and Lawrence*, 304.

Bourgougnon, A., *Physical Problems and their Solutions*, 127.

Bunsen, Robert Wilhelm, *Nichols*, 310.

Byerly, W. E., Tarleton's *An Introduction to the Mathematical Theory of Attraction*, 191.

C.

Calorimeter, A new respiration, *Atwater and Rosa*, 129, 214.

Carhart, Henry S., An absolute determination of the E. M. F. of the Clark cell, 288.

Cells, electrolytic, a photographic study of, *Ramsey*, 189.

Clark cell, an absolute determination of the E. M. F. of the, *Carhart and Guthe*, 288.

Clebsch's, on a theorem of, *Saurel*, 300.

Conservation of energy in the human body, and a new respiration calorimeter, *Atwater and Rosa*, 129, 214.

D.

Dorsey, H. G., The effect of magnetism on the elasticity of rods, 116.

E.

Electromotive force of the Clark cell, an absolute determination of, *Carhart and Guthe*, 288.

Electrolytic break, the Wehnelt, *Humphreys*, 30.

Electrolytic cells, a photographic study of, *Ramsey*, 189.

Elasticity of rods, the effect of magnetism on, *Stevens and Dorsey*, 116.

F.

Foster, William, Jr., The hydrolysis of stannic chloride, 41.

Freezing and boiling water simultaneously, *Quick*, 121.

Freezing points of aqueous solutions of non-electrolytes, *Loomis*, 257.

G.

Gage, O. A., An investigation of the magnetic qualities of building brick, 304.

Goodwin, H. M., *The Fundamental Laws of Electrolytic Conduction*, 316.

Gressman, George W., The electrical resistance of lead amalgams at low temperatures, 20.

Guthe, Karl E., An absolute determination of the E.M.F. of the Clark cell, 288.

H.

Hankel, Wilhelm Gottlieb, *Nichols*, 58.

Hartman, Leon W., The photometric study of mixtures of acetylene and hydrogen burned in air, 176.

Hastings, Charles S., *Text-book of General Physics*, 313.

Hathaway, Arthur S., Tait's *Scientific Papers*, 123.

Holman, Silas W., *Matter, Energy, Force and Work*, 59.

Humphreys, W. J., The Wehnelt electrolytic break, 30.

Hydrogen and acetylene, photometric study of mixtures of, burned in air, *Hartman*, 176.

Hydrolysis of stannic chloride, *Foster*, 41.

Hyndman, H. H. Francis, *Radiation*, 252.

I.

Interferometer study of radiations in a magnetic field, *Shedd*, 1, 86.

International congress of physicists, 255.

Iron wires, resistance offered by, to alternating currents, *Merritt*, 294.

K.

Kester, Fred. E., A method for the study of phosphorescent sulphides, 164.

L.

Lawrence, H. E., An investigation of the magnetic qualities of building brick, 304.

Lead amalgams at low temperatures, the electrical resistance of, *Gressman*, 20.

Loomis, E. H., On the freezing points of aqueous solutions of non-electrolytes, 257.

M.

Magie, William Francis, The specific heat of solutions which are not electrolytes, 65.

The Second Law of Thermodynamics, 316.

Magnetic field, interferometer studies of radiations in, *Shedd*, 1, 86.

Magnetic qualities of building brick, *Gage and Lawrence*, 304.

Magnetic shielding effect of tri-lamellar spherical and cylindrical shells, *Wills*, 193.

Magnetism, effect of, on the elasticity of rods, *Stevens and Dorsey*, 116.

Merritt, Ernest, The resistance offered by iron wires to alternating currents, 294.

N.

Nichols, E. L., Bourgougnon's *Physical Problems and their Solutions*, 127.

Gustav Wiedemann, 57.

Harper's Scientific Memoirs, 64, 316.

Ostwald's *Klassiker der Exakten Wissenschaften*, 127.

Robert Wilhelm Bunsen, 310.

Todd's *Stars and Telescopes*, 192.

Watson's *Practical Physics*, 315.

Wilhelm Gottlieb Hankel, 58.

Non-electrolytes, freezing points of aqueous solutions of, *Loomis*, 257.

P.

Phosphorescent sulphides, a method for the study of, *Kester*, 164.

Photometric study of mixtures of acetylene and hydrogen burned in air, *Hartman*, 176.

Q.

Quick, R. W., On freezing and boiling water simultaneously, 121.

R.

Radiations in a magnetic field, interferometer studies of, *Shedd*, 1, 86.

Ramsey, Rolla R., A photographic study of electrolytic cells, 189.

Resistance, electrical, of lead amalgams at low temperatures, *Gressman*, 20.

Resistance offered by iron wires to alternating currents, *Merritt*, 294.

Respiration calorimeter and experiments on the conservation of energy in the human body, *Atwater and Rosa*, 129, 214.

Rosa, E. B., A new respiration calorimeter, and experiments on the conservation of energy in the human body, 129, 214.

S.

Saurel, Paul, On a theorem of Clebsch's, 300.

Shedd, John C., Interferometer studies of radiations in a magnetic field, 1, 86.

Solutions, aqueous, of non-electrolytes, the freezing-points of, *Loomis*, 257.

Solutions not electrolytes, specific heat of, *Magie*, 65.

Specific heat of solutions which are not electrolytes, *Magie*, 65.

Stannic chloride, hydrolysis of, *Foster*, 41.

Stevens, J. S., The effect of magnetism on the elasticity of rods, 116.

Stewart, O. M., Hyndman's *Radiation*, 252.

T.

Tait, P. G., *Scientific Papers*, 123.

Tarleton, Francis A., *An Introduction to the Mathematical Theory of Attraction*, 191.

Temperatures, low, the electrical resistance of lead amalgams at, *Gressman*, 20.

Theorem of Clebsch's, *Saurel*, 300.

Todd, David B., *Stars and Telescopes*, 192.

W.

Water, boiling and freezing simultaneously, *Quick*, 121.

Watson, William, *Practical Physics*, 315.

Wehnelt electrolytic break, *Humphreys*, 30.

Whitman, Frank P., Hastings and Beach's *Text-book of General Physics*, 313.

Wiedemann, Gustav, *Nichols*, 57.

Wills, A. P., On the magnetic shielding effect of tri-lamellar spherical and cylindrical shells, 193.

THE

PHYSICAL REVIEW

A JOURNAL OF EXPERIMENTAL AND
THEORETICAL PHYSICS

CONDUCTED BY
EDWARD L. NICHOLS, ERNEST MERRITT,
AND FREDERICK BEDELL

VOL. X

PUBLISHED FOR CORNELL UNIVERSITY
THE MACMILLAN COMPANY
NEW YORK LONDON
BERLIN : MAYER AND MUELLER
1900

THE NEW YORK
PUBLIC LIBRARY
203940
ASTOR, LENOX AND
TILDEN FOUNDATIONS
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THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

CONTENTS OF VOLUME X

L. JANUARY, 1900

PAGE

A Determination of the Nature of the Electric and Magnetic Qualities and of the Density and Elasticity of the Ether. I. R. A. FESSENDEN. 1

Polarization and Internal Resistance of the Copper Voltameter. B. E. MOORE 34

A New Method of Comparing Two Self-Inductances. H. V. CARPENTER . . 52

New Books: *Wüllner:* Lehrbuch der Experimental Physik; *Ostwald's* Klassiker der Exakten Wissenschaften, Nos. 99 and 100; *Lengfeld:* Inorganic Chemical Preparations; *Hillyer:* Laboratory Manual 63

LI. FEBRUARY, 1900

The Influence of Electrification on the Surface Tension of Water and Mercury. ERNEST MERRITT and SAMUEL J. BARNETT 65

An Objective Representation of the Hysteresis of Iron and Steel. KNUT ÅNGSTRÖM 74

A Determination of the Nature of the Electric and Magnetic Quantities and of the Density and Elasticity of the Ether. II. REGINALD A. FESSENDEN. . 83

Thermodynamic Notes (No. 10); Revision of Some Thermal Data Concerning Benzene. K. TSURUTA 116

An Efficient Boyle's-Law Apparatus. W. J. HUMPHREYS 123

New Books: *Scheiner:* Strahlung und Temperatur der Sonne; *Gerland und Traumüller:* Geschichte der Physikalischen Experimentierkunst 126

LII. MARCH, 1900

On the Metabolism of Matter in the Living Body. EDWARD B. ROSA 129

A Dissociation Theory of the Electric Arc. C. D. CHILD 151

The Effect of Magnetization on the Elasticity of Rods. II. JAMES S. STEVENS 161

The Influence of Temperature, Pressure, Used Solutions, and Size of Anodes on the Deposit of Silver Voltameters. J. F. MERRILL 167

A Lecture Demonstration in Wave Motion. WILL. C. BAKER 175

An Apparatus for the Determination of the Coefficient of Expansion of Air at Constant Pressure. C. F. ADAMS 178

On the Change in Length of Soft Iron in an Alternating Magnetic Field. LOUIS W. AUSTIN 180

New Books: *Wentworth and Hill:* Text-book of Physics; *Franklin and Williamson:* Alternating Currents; *Aimé:* De l'influence de la pression sur les actions Chimiques; *Duhem:* Mechanique Chimique; *Miller:* American Telephone Practice 187

LIII. APRIL, 1900

The Acoustic Analysis of the Vowels from the Phonographic Record. LOUIS BEVIER, JR. 193

Note on the Variation of the Specific Heat of Water Between 0° and 100° C. H. L. CALLENDAR and H. T. BARNES 202

The Magnetic Molecular Susceptibility of Salts of the Iron Group. O. LIEBKNECHT and A. P. WILLS 215

Gibbs' Thermodynamic Model. W. P. BOYNTON 228

On the Temperature of the Acetylene Flame. EDWARD L. NICHOLS . . 234

Note on the Law of Distribution of Velocities Among Gas Molecules. N. D. C. HODGES 253

New Books: *Crew:* Elements of Physics; *Watson:* Text-Book of Physics . . 254

LIV. MAY-JUNE, 1900

The Transmission of the Ionized Exhalations of Phosphorus Through Air and Other Media. C. BARUS 257

An Hermetically Sealed Type of Clark Standard Cell. H. T. BARNES . . . 268

Concerning Thermal Conductivity in Iron. EDWIN H. HALL 277

Spark-Length of an Electric Influence Machine as Modified by a Small Spark from the Negative Side. W. J. HUMPHREYS 311

Index 317

Volume X. *January, 1900.* *Number 1.*

THE
PHYSICAL REVIEW.

A DETERMINATION OF THE NATURE OF THE ELECTRIC AND MAGNETIC QUANTITIES AND OF THE DENSITY AND ELASTICITY OF THE ETHER. I.

Reginald Aubrey Fessenden.

Part I.

Introduction.
Dimensional Formulæ and Qualitative Mathematics.
Previous Work.
Résumé of some Earlier Papers.
Reduction of the Number of Possible Theories.

Part II.

The Critical Relation.
The Variation of Permeability with Magnetic Intensity.
The Solution.

Part III.

Corroborative evidence : A, Permeability ; B, Capacity.
Determination of the Density and Elasticity of the Ether.
Summary.
Conclusions.

Appendices.

A. Experimental work to determine the relation between H and μ.
B. Abstract of Previous work in search of the Fourth Equation.
C. Nomenclature, Symbols and Units.

PART I.

Introduction.

THE advance of science is not a continuous sequence of successes, with, day by day, verifying experiment hot following brilliant theory and completer theory advented by suggestive experiment. Here and there in the field are positions where the pause seems absolute and where the expert of a single tool, after a lifetime of unsuccessful toil, may dejectedly believe the years to have passed in profitless endeavor.

Yet the work is not wasted, it is only that what was looked for to appear as kinetic is stored as potential; in due time this will take the desired form, and add mightily to the velocity of the advance. The forging of new weapons of attack, the running of parallels, the exact determination of the difficulties to be overcome, are a part of the advance as much as is the actual motion over the field, and in the end cause no delay.

When Maxwell wrote the last word of his electromagnetic theory he had reached a point beyond which we have yet to pass. His theory has been confirmed by the experiments of Rowland, Lodge and Hertz; it has been broadened and generalized by the masterful and masterly labors of Heavyside; it has been elucidated by the acute and penetrating analysis of Poincaré; Helmholtz, Fitzgerald and other of the great physicists have aided in its development, and yet we have made no marked advance as regards our knowledge of the nature of the electric and magnetic quantities.

And this is the more remarkable when we consider the great number of striking discoveries which have been made since then. Even in the direction considered by Faraday as perhaps the most promising point of attack, that is the behavior of vacuum tubes, the magnificent results obtained by Crookes, Thomson, Lenard and Röntgen are as yet not capable of being utilized for our purpose.

There has been no lack of growth of increase of mathematical power. Our knowledge of vortex motion, for instance, has been brought, by the papers of Thomson, Kelvin, Fitzgerald and Hicks, to a point which but a few years ago could hardly have been hoped for. In addition, the genius of Heavyside has placed at our disposal an engine of surpassing strength for physical work, though perhaps at present there will be more of us to respectfully admire it than to attempt to use it.

It will be of interest to briefly review the present status.

We can conceive of the action of one quantity of electricity upon another quantity of electricity, of one quantity of magnetism upon another quantity of magnetism, of a moving quantity of electricity upon a quantity of magnetism, and of the motion of an electromagnetic wave, as taking place in the free ether, in space entirely devoid of matter. Under these circumstances we have no lack of perfectly satisfactory theories. The emission theory, the elastic solid theory, the rotational-elasticity theory, all these give complete solutions. It is only when we come to consider the effect of matter that we meet with any difficulty.

Corresponding to the particular theory which we may choose to adopt, we have to assume a theory of the constitution of matter, and no theory has as yet been proposed which does not break down at one or more points.

The theories may be divided into two great classes, those which make the virtual density of the ether the same in all media, which is equivalent to making the magnetic permeability the density of the ether, and those which make the virtual elasticity of the ether the same in all media, which is equivalent to making the specific inductive capacity the density of the ether.

In the first of these classes are the theories of Fresnel, Kelvin and Fitzgerald.[1] In the second class are the theories of Neumann, MacCullagh and Larmor. Several theories of each class have been treated of with great power by Heavyside in his Electro-Magnetic Theory.

The point where it is generally conceded the theories of the first type break down, is in satisfying the conditions of continuity at the interface of different media.[2]

The second type of theories escapes, as Larmor has shown, from this difficulty. But Heavyside has pointed out, in the case of some of the theories of this class, that a new difficulty arises, in that the form of the equation expressing the loss of energy in a conductor due to its resistance is incompatible with experiment. And this defect, it seems to me, must exist in all theories of this class, including the one so admirably developed by Larmor.

[1] On a Hydro-Dynamic Hypothesis as to Electro-Magnetic Action, Proc. Roy. Dublin Soc., March 25, 1899.

[2] In this connection it may be asked if in the case of iron the outermost layers of atoms are magnetic (Ewing's experiments on joints seem to show that they are not), and if it may not possibly be found that there is an absorption of light on reflection from a magnetized pole piece which is a function of the rotation impressed.

There is thus apparently nothing to choose between the two classes of theories. Each class seems capable, by proper development where this has not already been done, of expressing practically all of our experimental facts, and each breaks down at one single point.[1]

There is no obvious reason why the theories should have been restricted to these two classes, for whilst, as Maxwell pointed out, the ether certainly has inertia, it does not follow that either permeability or capacity is its density. The fact remains, however, that theories of these two types are the only ones which have given even approximately satisfactory results, and it will later be shown why this must be so.

The problem divides itself into two parts, the nature of the quantities involved, and their configuration. Of both of these we are entirely ignorant. A knowledge of the former means a full solution of the problem of the nature of electricity and magnetism, in the same sense, in which we say that we know the nature of sound when we have discovered that it is produced by waves in a medium.[2]

Knowing the nature of the electric and magnetic quantities, there is a choice of suitable configurations, any one of which will be entirely satisfactory. Since, however, the true configuration must also account for gravitation and inertia, it would be futile to develop the theory to any extent in connection with a configuration which did not at the same time give promise of results in this direction also.

In the present paper the nature of the electric and magnetic quantities will be determined, and a configuration will be indicated which may serve us for the present.[3]

[1] It is possible that each of the perfected theories of each type may represent an actual material universe, the two universes possibly interpenetrating each other. We, for the present at least, are concerned with but one, and which one has not yet been shown.

[2] We have of course no idea what a wave is; we figure it as motion in a certain way, but the ultimate motion may be something entirely different; it may, for instance, be some complicated form of rotary motion.

[3] The determination of a configuration which will explain, in addition, the nature of gravity, must be left to the future, as there appears to be at least two possible solutions and an additional independent physical relation must be discovered before we can say with certainty which is correct.

It will be shown that the number of electric and magnetic relations which can be expressed in terms of the electric, magnetic and mechanical quantities,[1] is not sufficient for a solution. There are four unknown quantities and but three independent phenomena. Hence some new relation must first be discovered.

This, it will be shown, is a relation between the magnetizing force, H and the magnetization, I, expressed by the formula

$$\frac{dI}{dH} = c\frac{I^2}{H^2}$$

or, expressed in words, "For all magnetic substances, the rate of variation of magnetization with magnetizing force is proportional to the square of the magnetic susceptibility."[2]

It is shown that this leads to a solution, which is confirmed by additional evidence in the shape of other new relations. From one of the latter, data are obtained from which a determination of the density and elasticity of the ether is made.

It thus happens that, exactly a century after the discovery of Volta, we are permitted a knowledge of the nature of the electric current.[3]

Dimensional Formulæ and Qualitative Mathematics.

Fourier's statement[4] with respect to dimensions is: "It must now be remarked that every undetermined magnitude or constant has one dimension proper to itself and that the terms of one and the same equation could not be compared if they had not the same exponent of dimensions. This consideration * * * is derived from primary notions on quantities, for which reason, in geometry and mechanics, and is the equivalent of the fundamental lemmæ which the Greeks have left us without proof."

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[1] By mechanical quantities is meant quantities which are known functions of length, mass, and time, only.

[2] Or translated into the elastic problem, it behaves like rubber. See Molecular Physics, Franklin Inst. Proc., Feb., 1897.

[3] This paper was completed in July, 1898, but has been held for consideration until now. It originally contained a section on the methods of employing qualitative mathematics for the discovery of new phenomena, and the working of physical problems. On consideration this has been omitted as not being in a sufficiently developed state.

[4] Theory of Heat: Freeman's translation, p. 128.

Dimensional formulæ were used extensively by Maxwell, who says :[1] "When a given unit varies as the *n*th power of these (three fundamental) units, it is said to be of the *n*th dimensions as regards that unit." "In all dynamical sciences it is possible to define these units (of different kinds of quantities to be measured) in terms of the three fundamental units of length, mass and time."

In several instances, however, for example in treating of viscosity,[2] he suppresses one of the dimensions. In the case instanced he finds, after so doing, that the dimensions of viscosity, measured in the kinematic way, are L^2/T, whilst he has previously shown that measured according to the dynamic method its dimensions are M/LT. The former dimensions are arrived at by taking the unit of mass as the mass of unit volume of the substance considered.

This suppression of one or more of the fundamental dimensions in the formula for a quantity seems to me to be, whilst no doubt perfectly legitimate, (since we have a right to make what rules we find convenient, provided we are consistent in our use of them), a considerable departure from what I take to be Fourier's idea. For if we suppress dimensions, then, instead of Fourier's "one dimension proper to itself," a physical quantity may have an infinite number of dimensional formulæ, the one we use depending upon which units we choose and which of those chosen we suppress.

We find numerous instances of this dropping of dimensions elsewhere. For example, in Everett's book on C. G. S. units we have the following equation,[3]

$$M = L^3/T^2.$$

Whatever Fourier's conception really was, at the present time dimensions are generally treated as being merely an affair of units, any particular dimensions appearing or disappearing as we vary the units with which we work. It is evident that such a system, in which the formula for any particular quantity may be varied at pleasure, can be of little use as a method of mathematical investigation, and some of our most able mathematical physicists have expressed an opinion to that effect.

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[1] Elect. and Mag., Vol. I., p. 2.

[2] Theory of Heat, p. 299.

[3] C. G. S. System of Units, p. 73.

J. J. Thomson says,[1] "It may be well to state at the outset that the dimensions of the electrical quantities are a matter of definition and depend entirely upon the system which we adopt." Also, "A theory of dimensions cannot tell us what electricity is; its object is merely to enable us to find the change in the numerical measure of a given charge of electricity or any other electrical quantity, when the units of length, mass and time are changed in any determinate way."

Webster is equally definite. To quote[2]: "The absolute dimensions of either factor are arbitrary. Attempts have been made to settle the real dimensions of x and μ, but they are evidently based upon misconceptions of the theory of dimensions."

The weight of authority is very greatly in support of the definition of dimensions as merely arbitrary formulæ, expressing the relation between certain quantities and the units we chose to work with.

There is, however, a branch of mathematics of great power and capable of extensive and important application in physics, more especially as a means of discovering new phenomena, in which the formula assigned to any quantity represents its ultimate nature, whatever that may be. Of course, this does not imply that we *know* what its ultimate nature is, but this does not invalidate results obtained by its means. ML^2 is motion of inertia, no matter what M is ultimately, and nothing which we may find out about mass can change this fact. M may contain three or four factors, but, still, the product of these factors, which we represent by M, will when multiplied by L^2 continue to represent moment of inertia.

In view of the opinions and definitions of the eminent naturalists quoted above as to the meaning of the term dimensions it would be improper for me to apply that term in any other sense. I shall therefore call the formula expressing the ultimate nature of a quantity its "quality," and this branch of mathematics "qualitative mathematics." This term seems suitable, for the method is used to make deductions with reference to the *qualities* of a physical entity, as distinguished from other branches which concern themselves with

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[1] Elements of Elect. and Mag., p. 447.

[2] Theory of Elect. and Mag., p. 418.

quantity or direction. The term has a precedent in science, for we speak of quantitative and qualitative chemical analysis.

The difference between the dimensional formula and the qualitative formula or quality of a thing is that, according to the definition of the writers quoted above, the *dimensions* "are arbitrary," are "merely a matter of definition and depend entirely upon the system of units we adopt," whilst the *quality* is an expression of the absolute nature and never varies, no matter what system of units we adopt. For this to be true, no qualities must be suppressed.[1]

All the phenomena of physics with which we are acquainted deal with the following quantities only :[2]

Name.	Symbol.
1. Quantity of gravity,	G
2. Quantity of electricity,	Q
3. Quantity of magnetism,	P
4. Gravitational coefficient,	γ
5. Specific inductive capacity,	κ
6. Magnetic permeability,	μ
7. Mass,	M
8. Length,	L
9. Time,	T

The defining equations being

$$\frac{G^2}{\gamma} = \frac{Q^2}{\kappa} = \frac{P^2}{\mu} = \frac{ML^3}{T^2}.$$

Any physical phenomena as, for instance the vibration of a stretched string, may be concerned with all these quantities, for it may be so long and heavy that gravitational force may be appreciable, it may have an electric charge on it, and it may be magnetized and vibrating in a medium of a given permeability and specific inductive capacity. If these quantities were not all expressible in terms of one or more of three fundamental quantities we should need, therefore, no less than six distinct experiments to completely define each phenomenon. As it is, however, we need but three. From this also it follows that, if the phenomenon can exist whilst concerning but three of the above eight quantities, the effect of the introduction of the other quantities is merely to add terms of the same quality as the original one.[1]

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[1] As an example of the difference between dimensional and qualitative formula we may take the result above referred to as contained in Everett's C. G. S. Units, p. 73. It is there shown that by taking the unit of mass as that which produces unit acceleration at unit distance, the dimensions of mass become $M = L^3/T^2$. It is evident that so soon as we attempt to use these dimensions in other than gravitational problems we must get incorrect results owing to the limitations we have placed upon ourselves. In the qualitative system, however, M is always M, if we take M as a fundamental unit, and if we take any other unit M is always represented by terms which are equal to M, so that no matter what branch of physics we work in we always use the same formula.

[2] This statement is as incapable of proof, and for the same reasons, as the statement embodying the law of the conservation of energy.

As an example, let us take a bead on a string. Its time of vibration depends upon, amongst other things, its mass. If it is charged the effective mass is increased, as is shown by J. J. Thomson (Application of Dynamics to Physics and Chemistry, p. 32), by an amount $2/15(\mu Q^2/a)$ (Heavyside says $\frac{1}{3}$), where Q is the charge, a the radius of the sphere, and μ the permeability of the medium. Here $\mu Q^2/a$ has the dimensions M. If the pendulum had a magnetic charge, there would be a term $P^2 x/a$, and if gravity had also to be taken into account we should have a term $G^2 x\mu/\gamma a$.

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[1] A symmetrical system might also be made by introducing coefficients analogous to κ and μ into the equations connecting M and G, as was done by the writer (Elect. World, May 18, 1895). Also (as was done, *ibid.*) by introducing Ψ, the symbol for energy, and making it the third fundamental unit with L and T. For the present the above will be used.

In addition to these, however, we have terms which express configuration and terms which denote direction or operations. In brief, this branch of mathematics may be taken as a very precise logic.

As an example of a term depending upon configuration we may take the case of temperature. The quality of temperature, as may readily be shown, is

$$\theta = ML^2/T^2 o,$$

where θ represents the quality of temperature and o the quality of an atom, temperature being the amount of kinetic energy per atom. It will thus be seen that whenever we have a qualitative equation in which some physical quality is shown to depend upon temperature, that quality must also be a function of the atomic weight, atomic volume, atomic valency or atomic magnetic moment.

Here the term atom is defined as that configuration upon which depends the various properties which distinguish the atom from the surrounding ether. By configuration is meant not only the relative positions, but also the relative lengths and velocities of the different parts of the atom. When we know these, however, we can split o up into separate terms. Even at present we can do this to some extent. For instance, from Faraday's law of electrolysis we have $M/$ atomic weight $= Q/$ valency. From which we get atomic weight $+ T =$ valency, which is interesting in view of Hicks's work.

Previous Work.

Maxwell, Kelvin and other writers have given dimensional formulæ for the electric and magnetic quantities, in which the dimensions of x and μ were suppressed. So far as I am aware the complete dimensions or qualities in terms of L, M, T, x and μ were first given by Rücker, in the *Philosophical Magazine*, February, 1889.[1]

It is also pointed out in the above-mentioned paper that if the electric and magnetic quantities are of the same nature as those met with in mechanics, then, since the ratio of the dimensions in terms of x to those in terms of μ is a power of $x^{½}$, $\mu^{½}$, L and T and the two expressions must be identical, $x^{½}\ \mu^{½}$ must equal T/L, which is Maxwell's equation.

Lodge has given, in his Modern Views of Electricity, p. 702, 1889, expressions for the electric and magnetic quantities in terms of L, M, T, x and μ, and a further list, taking μ to be a density. He points out that this is an assumption, and refers to Fitzgerald's suggestion that both x and μ may have the same dimensions.

Williams (*Phil. Mag.*, Sept., 1892) published a very valuable paper, in which, making certain very natural assumptions, he arrived at the result that there are but two possible solutions, either μ is a density and x a compliancy, or vice versa.

The writer (*Electrical World*, May 18, 1895) showed that all the electrical and magnetic relations which had been exactly determined could be brought under three equations. From this it was deduced that, since there are four quantities concerned, an infinite number of theories was possible, all of which would be in agreement with the above mentioned relations. It was also pointed out that further experiments must be made to obtain a fourth equation.

Joubin (*Journal de Phys.*, Sept., 1896) came to the conclusion from an examination of the manner in which the exponents of L, M, T, x and μ vary when the electrical and magnetic quantities are written in terms of them, that μ must be a density and x a compliancy.

There are considerable gaps in the reasoning by which this result is reached. After reducing the number of possible theories to twelve, the author remarks that they are all impossible with the exception of the one which makes μ a density. No reason is given for this statement, which moreover is incorrect.

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[1] Though the fact that κ and μ should be included in the dimensions of these quantities had previously been pointed out, as I learn from Professor Rücker, by other physicists.

The writer (*Electrical World*, Jan. 2, 1897) pointed out that the most promising field of search for the fourth equation was amongst the following:

1. Hall's effect. 2. Kerr's electrostatic effects. 3. Fizeau's experiment. 4. Magnetic rotation of light and other outlying phenomena.

It was also pointed out that our knowledge of these phenomena is not at present sufficient to enable us to use them to obtain the desired equation.

Joubin (*Journal de Phys.*, Feb., 1897), following the method described in the paper just referred to, though apparently without knowledge of it, or of the warning given as to the risk of applying the method without further experiments, by considering certain formulæ which he takes as representing the phenomena of thermo-electricity, magnetic rotation of light and piezo-electricity, arrives at the conclusion that the result reached in his former paper is correct· There is however, little justification for the formulæ adopted to represent the phenomena considered, and with very little change the arguments advanced might be used to prove that it is x which is the density.

Abstracts from Some Earlier Papers.

In the Electrical World, May 18, 1895, the writer pointed out that all the dynamical relations which have been accurately formulated connecting the electric and magnetic quantities are contained in the first three of the following equations:

(1) $$Q/P = x^{1/2}/\mu^{1/2},$$

(2) $$QP = ML^2/T,$$

(3) $$x^{1/2}\mu^{1/2} = T/L,$$

(4) $$x^{1/2}/\mu^{1/2} = Z.$$

The first equation is obtained from Coulomb's law,

$$QQ'/L^2x = ML/T^2$$

and the corresponding magnetic formula

$$PP'/L^2\mu = ML/T^2.$$

The second equation is from Faraday's law, that the time rate of change of induction P/T, is a measure of the voltage, voltage multiplied by current giving power, *i. e.*,

$$P/T \times Q/T = ML^2/T^3.$$

The third equation is obtained from Weber's law, that a circuit carrying a current is equivalent in its magnetic effect to a magnetic shell whose moment is equal to the area enclosed by the contour of the circuit, *i. e.*,

$$Q/T \times \mu \times L^2 = PL,$$

by substituting for Q/P its value in terms of x and μ, as given from the first equation.

This third equation is known as Maxwell's equation.

It will be seen that we have four unknown quantities, Q, P, x and μ, whilst we have but three equations. Put $x^{1/2}/\mu^{1/2} = Z$ for a fourth equation, and solve—we get,

$$Q = M^{1/2}LZ^{1/2}/T^{1/2}, \tag{5}$$

$$P = M^{1/2}L/T^{1/2}Z^{1/2}, \tag{6}$$

$$x = TZ/L, \tag{7}$$

$$\mu = T/LZ. \tag{8}$$

By giving different values to Z we obtain all possible theories of electricity, *i. e.*, all theories which do not conflict with the known laws of electricity and magnetism.[1]

For example, let $Z = L^2T/M$.

On substituting we get

$$\begin{array}{ll} Q = L^2 & F = M/LT^2 \\ P = M/T & H = L/T \\ x = LT^2/M & \mu = M/L^3 \end{array}$$

where F is the drop in electrical potential per cm. or the voltivity, and H is the drop in magnetic potential per cm. or the gilbertivity.

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[1] So far as the ether is concerned any one of these theories is sufficient. It is only when we come to consider matter as well that we need a fourth equation.

This theory will probably be most familiar from Lodge's provisional use of it in his Modern Views of Electricity, as a means of giving a most lucid and satisfactory working conception of most of the phenomena of electricity and magnetism.

If we let $Z = M/L^2T$ we get the reciprocal theory to the above, *i. e.*, that in which x is the density and μ the compliancy.

A very considerable number of theories has been treated of by Heavyside in his Electromagnetic Theory, Vol. I. That on page 254 may be obtained by letting $Z = M/L^2T$, and is thus the reciprocal theory referred to above, making x the density and μ the compliancy, though this might not appear to be so at first glance, as that titanic mathematician is fond of using a special notation. The other difficulties which he refers to in connection with this theory, which in a modified form was first suggested by Kelvin, are no doubt those connected with the discontinuity of electric force at an interface.

Let $Z = ML^2T$, then,

$Q = ML^2$ = moment of inertia,
$P = 1/T$ = angular velocity.
$E = 1/T^2$ = angular acceleration.
$G = ML/T$ = angular momentum.
$\mu = 1/ML$ = reciprocal of moment of inertia.

We can get an idea of the mechanism of this theory by considering the motion of a pulley loosely fixed on a revolving shaft.

Let $Z = T^3/M$, then

$$Q = LT, \qquad P = ML/T^2,$$
$$x = T^4/ML, \qquad \mu = M/LT^2.$$

This theory makes the force with which an elastic string is pulled analogous to quantity of magnetism, the amount of stretch analogous to the gilbertance[1] and the elasticity of the string analogous to the magnetic permeability.

Reduction of the Number of Possible Theories.

So far we have not limited the values of Z in any way. It may contain any power, integral or fractional, of M, L and T.

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[1] Gilbertance, difference of magnetic potential.

But from examination of equations 5–8 we see that if Z does not contain M, then Q and P cannot be quantities of the same nature as those met with in mechanics, but must be square roots of such quantities. But if we have two charged bodies in space, having equal charges, the bodies themselves consisting of elastic hollow conductors, there being an indefinitely small hole in one of them, if we let the other conductor shrink so that it can be introduced into the indefinitely small hole in the other, the one conductor will now have a double charge. If these conductors are by themselves in space, it is difficult to see why, if Q were the square root of some dynamic quantity, the charge should be twice the original charge.

The same reasoning applies to the supposition that Q or P may be some other power than the first of one of the quantities we meet with in mechanics, and, hence, we arrive at the result that Q and P either contain M to the first power or else M is absent. This makes Z to contain M to the power $+ 1$ or $- 1$, as may be seen from equation 5.

As regards the power of L, it is difficult, as Williams has pointed out, to understand how a meaning can be attached to powers of L higher than the third. L is a length, L^2 is an area, and L^3 is a volume, but L^4, in conjunction with M to the first power, is not at present understandable. Consequently we may for the present suppose that L does not enter into the qualities of Q, P, x and μ to a power higher than the third. From examination of equations 5–8 we see that this means that L must enter into Z either as the $0 + 2$ or $- 2$ power.

But L cannot enter into Z as the 0 power, for since the capacity of a condenser has the quality xL and the permeance of a magnetic circuit the quality μL, either capacity or permeance must have the quality $M^x T^y L^0$, *i. e.*, must be entirely independent upon configuration. But we know that this is not true, for by changing the ratio of area to cross section we can change the capacitance or permeance and for a given change of ratio, the capacity is modified in the same way no matter what the substance or physical state. Consequently Z must contain L to the power $+ 2$ or $- 2$.

Again, from inspecting equations 5–8 we see that the exponent of M must have the opposite sign to the exponent for L, otherwise either capacitance or permeance will have the quality ML^3/T^4 and either Q or P will have no dimensions, T having the index given below.

As regards the exponent of T, in order that the quality of Q or P may not contain a fractional power of T, the index of T must be odd. If we reject all powers of T above the fourth as improbable, then T must enter into Z with the index $+1$ or -1. For if it had the index $+3$ or -3, then either the time rate of change of current or of voltage would be the fifth differential with respect to time of some quantity. This may, of course, be true, but can hardly be considered probable.

As we do not, in mechanics, meet with any quantity in which the exponent of M is of the same sign as the index of T, except when they are both zero, we finally arrive at the result that the quantity of Z is

$$M^{\pm 1}L^{\pm 2}T^{\mp 1}$$

or, substituting in equations 7 and 8,

either x *is a density and* μ *a compliancy, or vice versa.*

a result which is identical with that of Williams, though derived in a different manner.

It will be observed that in reaching this result we have not assumed *a priori*, that Q and P contain M to the 1 or 0 power, but have taken the additive property of Q and of P as our reason for considering such to be the case. To the writer this seems conclusive, but as will be seen later, the above proof is not at all essential to my reasoning, though it happens to agree with it, and I am myself persuaded of its validity.

PART II.

The Critical Relation.

We need a fourth equation. An account of some of the work done in past years, with a view to obtaining this equation, is given in an appendix. Instead, however, of examining phenomena, to see if they furnish the required equation, we may employ the converse method, *i. e.*, examine what equations are possible and see if there are in existence phenomena which fit them. This is advisable for another reason. It is held by some, to whom I have explained my ideas, that phenomena cannot be predicted by means of qualitative mathematics. I might reply to this that the method has as a matter of fact, been successfully used for this purpose, but for the present I prefer to abandon that ground and to rest my argument on the fact that, whether phenomena can or cannot be predicted by means of qualities, yet when the phenomena are discovered, how or by what means it matters not, the qualities can be determined from them. In equations 1–3 we have relations between Q and P, and κ and μ, but not between Q or κ and P or μ. Now the relations between Q and κ and P and μ involve P and Q respectively, giving merely the relations between the forces, fluxes and resistances. Consequently, we are thus naturally led to look for relations between Q and μ or P and κ.

As Q and μ belong to different systems, we must reduce them to the same. This we do by noting that Q/T is the current and that this is a measure of the gilbertance. But μ is a specific quantity, consequently we must take the gilbertance per unit length, or the gilbertivity, *i. e.*, $Q/LT = H$. Similarly $P/LT = F$ the voltivity.

We are, therefore, to look for a relation between H and μ or F and κ.

We may write F, H, κ and μ in terms of M, L, T and Z, and by giving different values to Z, see what relations should exist if that value of Z were correct. This was done and many relations were examined, and the result was again reached, by methods which I am not prepared to defend, that, as either κ or μ must be a density. For the present, assuming that the previous work of Williams and the writer is correct and that Z is either M/L^2T or L^2T/M, we obtain as a result, either

$$F = L/T \qquad \kappa = M/L^3$$
$$H = M/LT^2 \qquad \mu = LT^2/M$$

or

$$F = M/LT^2 \qquad \kappa = LT^2/M$$
$$H = L/T \qquad \mu = M/L^3.$$

From this we arrive at the conclusion,

Of κ *and* μ, *the one which is a compliancy will be an inverse function of the corresponding force.*

This is our touchstone, this relation. We know at once that the capacity of a condensor does not decrease when we increase the voltage on it, whilst we know that the permeance of a magnetic circuit does decrease when we increase the gilbertance. It remains to be proven that the rate of this change is that called for by the equation.

THE VARIATION OF μ WITH H.

The existence of a relation between H and μ has long been noticed, and a number of attempts has been made to formulate it exactly. Until recently the most satisfactory formula was that proposed by Lamont and Frölich. This is

$$1/\mu = a + bH.$$

This however is obviously incorrect for high values of induction, as it makes the total induction reach a limit, and Ewing's experiments show that this is not the case. A satisfactory formula was however proposed by Kennelly.[1]

He divides the total inductivity B, into two parts. One part due to the ether in the space occupied by the iron, and numerically equal to H which we will call B_e. This,[2] since the value of μ for the ether may be taken as 1, can increase indefinitely as H increases. The other part of B, that due to the iron, which we will denote by B_i, he calls the ferric inductivity. The ratio between H and B_i he calls the ferric reluctivity, and we will denote it by ν_i.

Then, with these definitions, Kennelly's formula is

$$\nu_i = 1/(\mu - \mu_e) = a + bH (= \text{numerically } 1/(\mu - 1).)$$

That this formula is correct within the limits of experimental error has been shown by Kennelly himself for a number of magnetic materials.

Figures 1, 2 and 3 show the relation between ν_i and H for various samples of iron, nickel and cobalt. It will be seen that in every case the main part of the curve is a straight line, as it should be.

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[1] Trans. Am. Inst. E. E., Oct. 27, 1891.

[2] I shall, to avoid considerable confusion and the use of a large number of additional symbols, take μ and κ as unity in vacuo.

This formula, it will be observed, does not hold for the first part of the curve. There is an abrupt bend at low values of H.

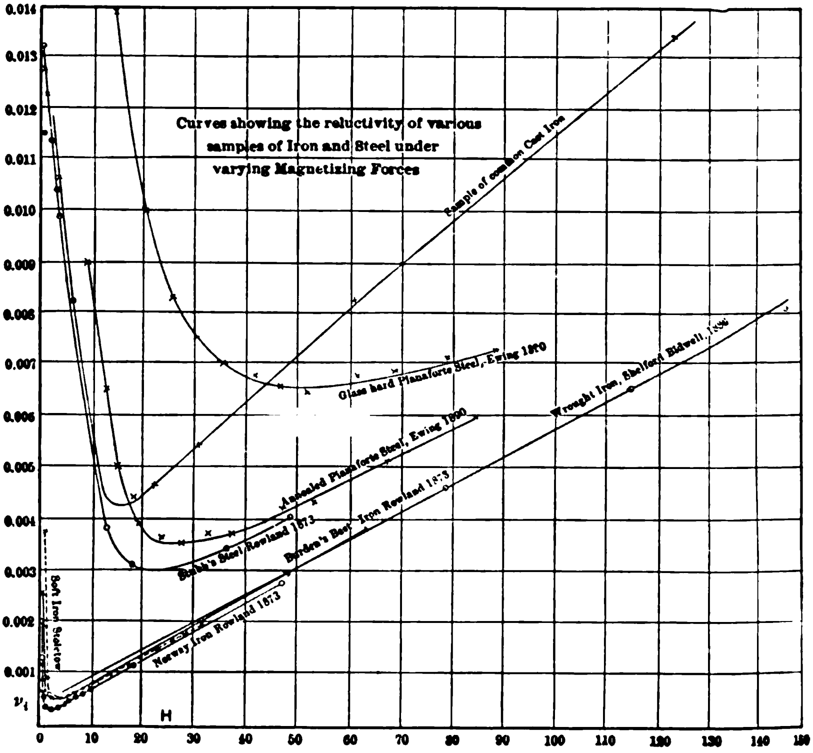

Fig. 1.

This bend however is not an essential part of the phenomenon; it disappears when the wire is tapped or has an alternating current passed through it. It seems to be due to the same cause as hysteresis, *i. e.*, molecular friction. It will be noted that those materials for which the hysteresis has a high value show the bend most pronouncedly.

Curve *A*, Fig. 5, shows the bend for a soft iron wire, and curve *B* is for the same specimen when an alternating current is passed through the wire. It will be seen that in the latter case the bend has entirely disappeared and the curve follows the formula throughout its length. The bend is therefore merely a superimposed phenomenon. The experiments from which these curves were drawn are those of Gerosa and Funzi.[1]

The question may, however, be asked, and has been asked, "Is not this relation one peculiar to iron and the other strongly magnetic

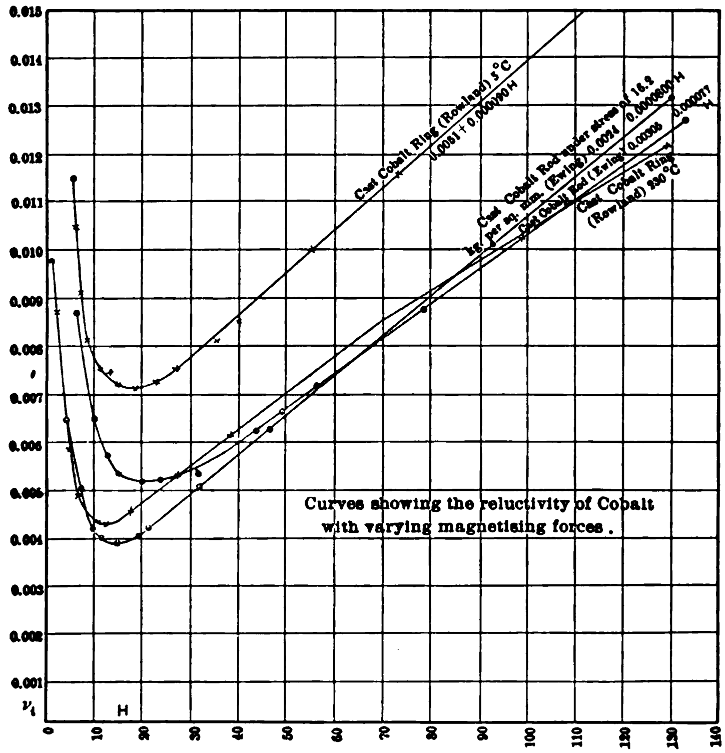

Fig. 2.

metals?" This answer is, that it is not, but appears to hold for all materials. For Steinmetz has shown,[2] that the same law holds for alloys of iron, such as the non-magnetic 10 % amalgam of iron for which the permeability is as low as 2. As, however, these alloys all had a strongly magnetic component, the writer decided to investigate the magnetic relations of some of the very slightly magnetic materials. Fortunately, however, in view of the great difficulty of the task, it was discovered that Silow had published some tests made on a solution of chloride of iron[1] and on plotting his results it was found that this substance also showed the same phenomenon, *i. e.*, the reluctivity decreases at first, but as *H* increases still further the reluctivity increases also. The subjoined table gives Silow's results, and it will be seen that they are of the same general nature as those obtained for iron though, possibly on account of the difficulties of the experiment, the curve is rather irregular. I propose to investigate this subject further.

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[1] Recond. del. Inst. Lombardo. See Ewing's "Magnetism in Iron and Other Metals," p. 319.

[2] Trans. Am. Inst. E. E., Sept. 27, 1892.

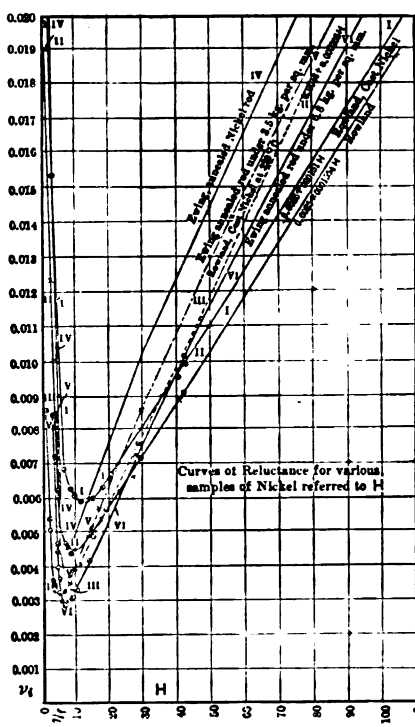

Fig. 3.

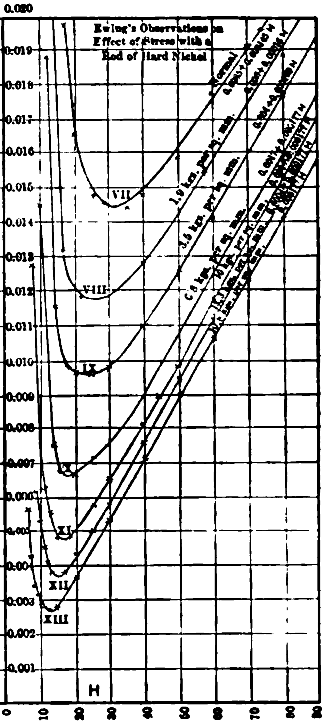

Fig. 4.

Plucker also noticed the same thing in the case of bismuth. A set of measurements, ranging from $H = 1100$ and up by another experimenter did not show the effect, but the test should be made with values of H from 1 to 20, as even in iron the permeability is practically constant when H is large.

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[1] Wiedemann, Annal., Vol. XI., 1880, p. 324.

Table Showing Variation of Susceptibility with H, for Ferric Chloride Solution.

H	Suscepti-bility.	H	Suscepti-bility.	H	Suscepti-bility.	H	Suscepti-bility.
1 15	0.000096	1.81	0.000142	2.40	0.000099	6.54	0.000065
1.35	0.000104	1.90	141	2.45	104	7.00	062
1 60	131	1.96	131	3.73	070	10.00	060
1.70	131	2.13	111	5.33	069	12.60	055

As the relation is shown to hold for substances for which ν_i has such different values as 0.003 (iron), 0.004 (hard steel), 0.1 (iron filings)[1] 0.9 (11 % iron amalgam) and 1000 (ferric chloride solution), it is extremely probable that the law holds generally, and that we are justified in making the following statement:

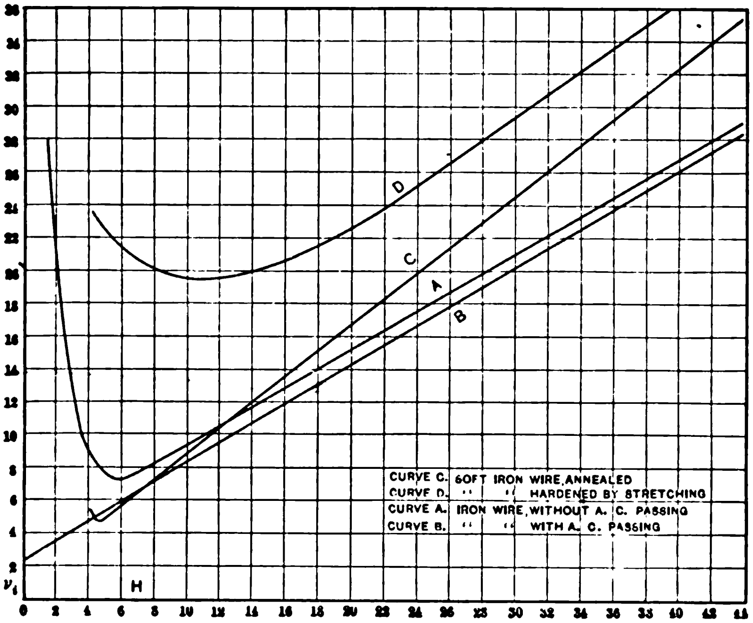

Fig. 5.

The relation between H and ν_i is given for all substances by the formula

$$\nu_i = a + bH$$

or, in a different form.

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[1] See Steinmetz, ibid.

For all substances,

$$dB_i/dH = aB_i^2/H_2$$

where, as will be seen later, *a* is a constant connected with the elastic properties of the material.

Having shown that the initial bend of the curve can be removed, and is thus merely a superimposed phenomenon, and that the law holds, in all probability, for all classes of substances, the question next arises, "Is the relation on exact one?"

Unfortunately we have at present no record of any very exact tests on iron or similar materials. That is to say, not exact as regards the connection between *B* and *H*, though no doubt amply precise enough for the purpose for which the tests were made. This is for the following reasons:

The experiments have been made on either straight wires, rings of iron wire, solid rings, solid rods or laminated rings. In all cases there are sources of error which were not taken into account in the experiments.

In the case of straight wires, the permeability varies with the induction, consequently the leakage varies also. This may give rise to errors of as much as 20% for high magnetizations.

In the case of rings of iron wire, no accurate results can be obtained, as the flux, having to pass from turn to turn through air, varies very greatly in density and the reluctance of the air spaces cannot be calculated owing to the varying permeability of the iron. Steinmetz has shown[1] that the variation from the mean induction in iron wire, wound somewhat differently, may be so great as to increase the hysteresis 900%. Such tests are not in any way a measure of the quantities supposed to be involved.

In the case of solid iron rods the induction cannot die away quickly owing to the eddy currents set up. Another still more serious error, especially with very thick rings, is the fact that the length of the magnetic circuit on the inside of the ring is less than that of the outside of the ring. If the ring is one foot in diameter, and the ring one inch thick, then this difference will amount to 16%. In other words the value of *H* in the inside of the ring is 16%

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[1] Trans. Am. Inst. E. E., pp. 699 and 701.

greater than that in the outside. As the permeability is not constant, the mean permeability is not the same as the permeability of the mean circumference, and this difference may amount to several per cent. Though this source of error was pointed out by Kennelly[1] I know of no published experiments in which this very important source of error has been taken into account.

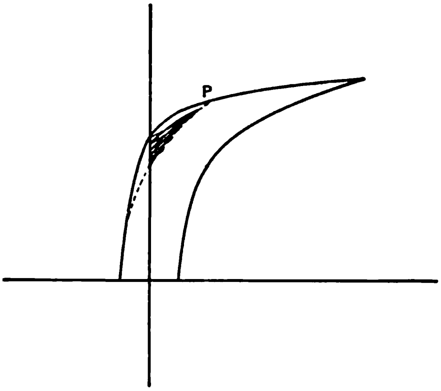

Fig. 6.

With solid iron bars tested in a permeameter there are quite a number of sources of error. But there is one thing alone which would vitiate all results obtained, especially for high values of B for which the permeameter is commonly supposed to give its most accurate readings, and that is that the formula used is incorrect.

The pull per sq. cm. is commonly given as

$$\text{Force} = (B^2 - H^2)8\pi.$$

It should be

$$B^2/8\pi\mu_m$$

where μ_m is the permeability of the medium, generally air, which flows in when the gap is made. The proof of this formula I have given elsewhere.[2] It is readily seen that the usual formula is wrong, for if we perform the following cycle:

1. Pull the bar, leaving the magnetizing coil in place.
2. Fasten the coil to the bar and let the bar come back to its original place.

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[1] Electrodynamic Machinery, p. 28.

[2] Frank. Inst. Lecture, Magnetic Design, April, 1899.

3. Move the coil to its original place.

It is self evident that if the pull be given by $(B^2 - H^2)/8\pi$ we will have a perpetual motion machine.

It is for the above reasons that the writer was reluctantly forced to the conclusion that there was no published work which could be relied upon to settle the point whether iron obeyed Kennelly's formula with an accuracy of one per cent. or less. A very elaborate and careful test was then made, the results of which are shown in Fig. 7, the method being described in an appendix, and the conclusion reached was that:

The sample of soft iron tested obeys the formula, with a maximum deviation from it of less than ¼%, this being the maximum amount of experimental error, for values above the bend of the curve.

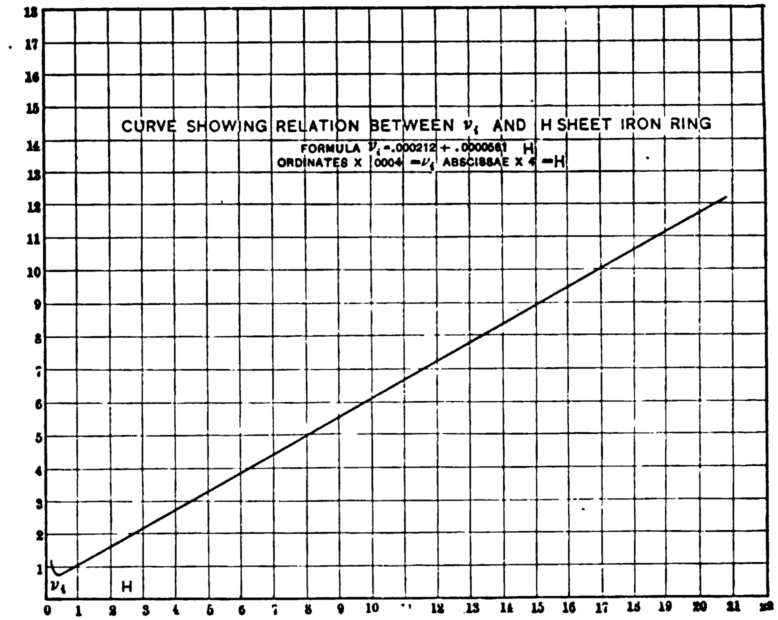

Fig. 7.

The Solution.

To resume, we have seen,

1. *That if the electrical quantities are of the same nature as those met with in mechanics, either x must be a density and μ a compliancy, or vica versa.*

2. *That, this being the case, either x must decrease as F increases, or μ decrease as H increases.*

3. *That whichever one of the two quantities, x or μ, decreases as the corresponding intensity increases, that one must be a compliancy.*

4. *That x does not decrease as F increases, but that μ does decrease as H increases.*

5. *That the rate of decrease is in agreement with the qualitative formula.*

6. *That therefore μ is of the nature of a compliancy.*

Z must therefore have the value M/L^2T. On substituting this value in equations 5–8 we obtain,

$$Q = M/T \qquad (12.)$$

$$P = L^2 \qquad (13.)$$

$$x = M/L^3 \qquad (14.)$$

$$\mu = LT^2/M \qquad (15.)$$

PART III.

Corroborative Evidence. A. Permeability.

Since, as we have seen

$$\nu_i = a + bH$$

and ν_i, being the reciprocal of a permeability, has the dimensions M/LT^2 then a must have the same quality.

Therefore a should vary with the elasticity of the material.[1] We can see that this is true by examining the curves of Fig. 1. Here the very hard materials, like pianoforte steel have large values of a. The soft materials, such as soft wrought iron have very low values of a. Intermediate materials, like cast iron have intermediate values of a.

Again if the material be hardened by stretching, a is increased. This is shown from the curves in Fig 5, taken from the experiments of Ewing.[2]

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[1] This may be questioned. I will not defend my statement here, as the main matter of importance is the fact that it *does* vary, whether the prediction of the phenomenon was or was not legitimate.

[2] Ibid., p. 304.

It will be noted that though *a* has changed markedly, *b* remains unchanged.

Again, M/LT^2 is of the same quality as a stress. Consequently we should expect *a* to change when a stress is applied to a wire. The curves shown in Figs. 4 and 2 show that this is true for both nickel and cobalt. These are drawn from Ewing's experiments,[1] and are taken from Kennelly's paper.[2] That the same thing holds true for iron may be seen from Ewing's experiments on that substance[3] though the experimental data are not very numerous.

In some of the curves given, it would appear as if *b* also changed slightly. This may be true, but it may also be due to the fact that when the bend is very gradual it is difficult to calculate the value of *b* from the data given, as the line remains inflected for some distance past the bend. The direction of the change of *a* should be noted as it gives us an idea of the nature of the stress.

M/LT^2 has the same quality as η in Steinmetz's formula for hysteresis, for both have the quality of work per unit volume. We might therefore expect η to change with *a*. The accompanying table[4] shows that this is true. Note especially the cases where the same material is tested in the annealed and hardened states. In each case *a* and η change, whilst *b* remains constant.

b has zero dimensions, and I have found that there is a relation between *a*, *b* and η as follows,

$$\frac{b\eta}{a} = 0.007, \text{ approximately.}$$

There is some evidence in favor of the idea that *b* has but two values for iron, one for soft iron and one for cast iron, with intermediate values when the material is intermediate in quality. It may therefore be related to the chemical constitution of the sample, and may represent the reciprocal of the number of molecular groups. There is not sufficient data at present available to enable us to decide the question.

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[1] Ibid.

[2] Ibid.

[3] Ibid., p. 179.

[4] Taken from Steinmetz's paper, Ibid.

Table Showing Relation between a, b and η for Various Substances.

Material.	α	β	η	$(\beta\eta/\alpha)\times 10^6$
Wrought Iron, Norway	.166	.054	0.00228	742
" " Common	.20	.055	0.00326	896
Sheet Iron	.275	.058	0.0035	738
" " Ferrotype, Commercial	.45	.050	0.00548	609
" " " Annealed	.337	.050	0.00458	679
" " Tinplate	.192	.055	0.00286	819
" " "	.321	.053	0.00426	703
Very Soft Iron Wire, Erwing	.20	.064	0.0020	640
Cast Iron No. 1	2.40	.094	0.0130	509
" " " 2	2.43	.094	0.0132	510
" " " 3	2.76	.095	0.0158	544
" " " 4	2.05	.097	0.0113	535
" " " 5	2.34	.095	0.0127	516
" " " 6	2.07	.097	0.0122	572
" " " 7	2.37	.098	0.0136	562
" " " 8	2.92	.095	0.0146	475
Cast Steel, Hardened	2.7	.054	0.0280	560
" " Annealed	.88	.054	0.00848	520
" " "	2.00	.091	0.012	546
" " Average of 5 samples	.736	.057	0.009	679
" " " "	.35	.054	0.005	771
Mitis metal	.30	.054	0.00428	770
Welded Steel, Tool Steel HI	8.0	.121	0.0748	1133
" " " " H	7.8	.105	0.0613	825
" " " " OH	1.9	.066	0.0267	927
" " " " O	1.54	.060	0.0270	1055
" " " " Sh	1.33	.060	0.0190	857
" " " " S	1.22	.058	0.0145	689
" " " " Nickel	1.00	.170	0.0122	207

Corroborative Evidence. B. Specific Inductive Capacity.

Since μ is a compliancy, x must be a density and its quality must be M/L^3

The fact that dense substances are generally highly refracting was first pointed out by Newton. Diamond and Faraday's dense glass are examples, as are also those liquids used for making determinations of the densities of heavy minerals. The following table shows the relation plainly. The data are taken from Everett's C.G.S. Units, and Landolt and Bornstern's tables. The tests on the glasses were made by Hopkinson; those on the gases by Ayrton and Perry.

Substance.	Density.	$\kappa - 1$.	$(\kappa - 1) / \text{Dens.}$
Glass.	4.5	9.1	2.0
Glass.	3.66	6.4	1.75
Diamond.	3.5	5.1	1.46
Glass.	3.2	5.85	1.82
Glass.	2.87	5.57	1.59
Sulphur.	2.	2.84	1.42
Benzol.	1.29	1.58	1.22
Rubber.	.92	1.32	1.43
CS_2.	.88	1.36	1.54
Paraffin.	.87	1.29	1.48
Kerosene.	.8	1.07	1.34
Petroleum Spirit.	.7	.92	1.31
SO_2.	.00269	.0052	1.93
CO_2.	.00195	.0023	1.18
Coal gas.	.00125	.0019	1.52
Air.	.00127	.0015	1.18

It will be seen that in the case of these substances, though the densities vary from 4.5 to 0.00125, or in the ratio of 4000 to 1, the added x is roughly proportional to 1.5 times the density.[1]

As this table might give a wrong impression, it should be stated that though the bodies taken were chosen at random, yet several substances, such as water and the alcohols, which behave anomalously in other ways, have been omitted. Some of the discrepancies may be accounted for. Thus I have found that the capacity of most of the organic oils is considerably decreased when they are carefully purified from organic acids, mucins and water. In the case of hydrogen the discrepancy may be due to the difficulty of measuring such a very small capacity.

But we cannot expect this relation to be more than an approximation for the following reason: The value of x varies with the periodicity and hence it is certain that, as in the case of the stretched string, the virtual mass must be made up of several terms. In fact, J. J. Thomson has shown that the capacity of a moving sphere is increased in the ratio $1/(1-4/15\,\mu x V)$. So that in addition to the density, the expression for x for a substance must contain terms depending upon the valency, the number of atoms per cc. the velocity of the atoms and the periodicity of the electric stress. These other terms may, it is conceivable in certain cases, be of larger numerical value than the original density term.

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[1] Newton gives a similar table (Opticks, Prop. X.).

Viewed, however, not as an attempt to find a law for refraction, but merely as a broad test between two rival theories of electricity, it is evident that x is related to the density and not to the elasticity of the substances.

As regards the variation of x in any particular substance with change of state, we have Newton's, Gladstone's, Lorenz and Lorentz's and Ketteler's formulæ. The first three of these do not agree satisfactorily with experiment. The last one does, probably because it contains more constants.

The series in Ketteler's formula can be expressed as a fractional power, and hence is of unity dimensions. This makes the refractive index to vary as the density, and since μ is practically unity for all the substances to which this formula applies, we get $x =$ density. Not much importance can be attributed to this result however.[1]

It is to be noted that the refractive index of a gas seems to vary in the direct ratio as its density. But the denser the gas the closer the atoms must be to each other, and the greater the molecular attraction forces, according to Van der Waal's law (or the writer's modification of it, *i. e.*,

$$\left(p + \frac{c}{v^{\frac{4}{3}}}\right)(v - b) = R\theta \text{)}.$$

Hence, also, in all probability, the greater the permeability. But, unfortunately, we know too little about these molecular actions at present to enable us to use the phenomenon in this connection.

Determination of the Density and Elasticity of the Ether.

If the ether has specific mass, and from the phenomena of light it must have some equivalent property, how would it affect the refraction of light?

In the first place we know that for most substances μ, the compliancy is practically the same as for the free ether. This means either that the elasticity of the free ether must be the same as that of the substance, or else that the total area of cross-section of the atoms is very small compared with the cross-section of the space they occupy to the exclusion of other atoms, *i. e.*, that the atoms have configuration virtually equivalent to that small framework structures of thin platinum wire interpenetrated by another substance.

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[1] These considerations suggest the following formula: $\frac{n^2 + 1}{\text{density}} + A = \text{Constant}$, where A represents terms depending on the velocity, valency of the atoms, etc.

If we are to look upon the atoms as possessing a structure whose actual cross-section is small compared with that of the space occupied by them,[1] their effect on the elasticity and density constants would be analogous to that obtained by adding to a vessel filled with oil a number of small structures built out of platinum wire. In this case the cross-section of the oil not being appreciably changed, the elasticity would not be altered to any extent, whilst the density of the platinum wire structures would be added to that of the oil. The resultant velocity of wave propagation in the space occupied by the combination would therefore be given by the formula

$$v = R/(d + D)$$

where d is the density of the oil, D the mass of the platinum wire structures contained in one cc. of space, and R the elasticity of the oil.

In this case, if the oil were set in motion with a velocity v, the velocity of the waves through it would be increased by the amount

$$vD/(D + d).$$

This fraction is of the same form as that called for by the aberration of light and Fizeau's experiment. Consequently if the previous work be correct and x is a density, then the fraction $(x - x_0)/$ density,[2] should be constant for all substances, at least to a first approximation.

Now the above table shows that this is approximately the case. There are exceptions, such as water and the alcohols, but by far the greater majority of substances obey this law. We know so little about the constitution of the atom that we cannot at present discuss the peculiar behavior of the exceptions. It is to be noticed that those bodies which do not agree with Maxwell's[1] law, that the refractive index for light is $x^{½}\mu^{½}$, are those which also disagree most widely from this rule that $(x - x_0)/$ density is a constant.

---

[1] The preceding table was prepared before I noticed the great discrepancy between Ayrton and Perry's and Boltzmann and Klemencic's values of κ for gases. If the values given by the latter be correct, the atoms may be comparatively solid, and we obtain 1.33 as the density and 12.10^{20} as the elasticity of the ether. As this is not yet absolutely certain, I leave the above as written.

[2] Taking κ for the ether as unity, this is numerically equal to $(\kappa - 1)/$ density.

The determination of an approximate value of the density of the ether from this formula is a very simple matter. Since in the formula, x is the specific inductive capacity of, or density of, both the stuff and of the space occupied by it, and we take the specific inductive capacity of the ether, according to our present system of units, as unity, then $x - 1$ is the density of the material in terms of the density of the ether. The average value of the constant is 1.5, therefore the value of the density of the material in terms of the density of the ether is 1.5 times its density in terms of water, *i. e.*, the density of the ether is 0.66, or two-thirds that of water.

From this, by means of the formula for the velocity of light, we may deduce the elasticity of the ether and find it to be

$$R = 6 \times 10^{20}.$$

As a converse proposition, it follows that to find the specific inductive capacity of any substance, all we have to do is to add to its specific mass the specific mass of the ether, and then divide the sum by 0.66. Thus, to get the specific inductive capacity of a mineral oil of density 0.5, we get

$$(0.5 + 0.66) \div 0.66 = 1.75 = x.^{2}$$

It will be noted that the values given for the constants of the ether lie midway between those generally given. Lord Kelvin's value for the elasticity[3] is 900, with a correspondingly small density.

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[1] I have previously pointed out that those substances which do not obey Maxwell's law act as negative uniaxial crystals in Kerr's experiment, whilst those obeying the law act as positive crystals, and that this indicates that those acting as negative crystals probably contain a certain per cent. of particles of substances of large specific induction capacity. —Elect. World, Jan. 2, 1897.

[2] Accepting Boltzmann's and Klemencic's results for κ we have, Mass of atoms in one c.c. — density of ether (1 — volume actually occupied by atoms) $= \kappa$. Hence, for spherical atoms not in the form of crystalline solids, we have the formula $(n^2 - 0.7)/$density $=$ constant.

[3] Trans. R. S. E., XXI., 60.

This value, however, can hardly be correct. For unless the atoms hold together by means of hooks of some kind, it would seem as if the stress when a bar of metal is sheared must ultimately come on the ether. In fact, the writer has shown[1] that the phenomena of rigidity, elasticity and tensile strength are quite well explained by supposing that the atoms are electric doublets and have charges on them of the same size as those called for by Faraday's law of electrolysis, and that the effects observed are of the same dimensions as those called for by this theory. But whether this theory be correct or not, it would seem that if the ether is the thing ultimately acted upon, it must be at least as rigid as the most rigid substance we know of, *i. e.*, the diamond, which has a rigidity of approximately 10^{14}. This calls for a minimum density of the ether of about 10^{-7}, or approximately that of air at 1/15 mm. pressure. Therefore, on the very lowest supposition the density of the ether in a vacuum tube must be immensely greater than that of the enclosed gas.

The other hypothesis, that held by Fitzgerald and by some other eminent naturalists, makes the elasticity of the ether to be about 10^{21} and its density at least 20. At first sight it would appear extremely reasonable to suppose that, if the atom be simply some configuration of the ether, the ether must be at least as dense as the densest atom. More especially since, as has been pointed out, both the fact that the permeability, *i. e.*, elasticity of the ether, is but very little affected by the presence of matter and the fact that, (since the fraction $(x - x_e)/$ density is approximately constant, *i. e.*) the density of the element is simply added to that of the ether, seem to show that the virtual cross section of the atoms is a small fraction (say one per cent. or less) of the space occupied by the atoms to the exclusion of other atoms. (Of course the actual space may be greater but in this case there must be some compensating device brought into play.)

But, on the other hand, it is not evident that there is any *necessary* connection between the density of the ether and the density of the atom. For it is very easy to see that if the atom consisted of some kind of a vortex in the ether, that owing to some property of the atom, say its energy of rotation, a strain might be set up in the ether proportional to its velocity squared. The total density of the atom might, therefore, be made up of two parts, one due to the density of the ether itself, which might be extremely small, and the other depending in some way on the configuration of the atom, this latter part being associated with a proportional quantity of gravity. Such an hypothesis, however small its justification, enables us to see that the ether *need* not be as dense as platinum.

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[1] Elec. World, August 8 and 22, 1891, more fully, Science, July 22, 1892, and March 3, 1893.

Again, we may take a vortex ether, such as has been treated of so powerfully by Fitzgerald and Kelvin. In this case we may conceive of space as filled with a tangle of vortices, the actual volume of the vortices being small compared with the space they occupy, this tangle of vortices being what we call the ether. If, by any action a new configuration is made to take place, and this is done in such a manner that it has the properties of matter, it may, at the same time, call for a crowding together of the vortices at that portion of space. In fact there are a number of, not at present improbable, suppositions which we might make for the purpose of showing that there is no *necessary* connection between the density of the ether and of the elements.

(*To be continued.*)

POLARIZATION AND INTERNAL RESISTANCE OF THE COPPER VOLTAMETER.

BY B. E. MOORE.

MEASUREMENTS of electrolytic polarization may be divided into two general classes. In the first class may be placed all the measurements made with the direct current. This class includes the greater part of the determinations. In the second class the alternating current has been employed. The development of this method is a comparatively recent one, but promises to yield extremely important results. This method has also afforded measurements of electrode capacity, designated by "polarization capacity"—an expression which first appeared in a contribution by Kohlrausch.[1] This term suggests that the phenomenon is limited to the electrodes and to the solution immediately adjoining them. The enormous values obtained for the capacities,[2] indicate strata so thin as to be scarcely designated as finite. Their values are a reminder of the Helmholtz charged strata theory. However, the Warburg theory, which makes it a current conduction, and at the same time recognizes that both the electrodes and the solution play important parts, is more satisfactory. The process at the electrodes is frequently complicated. Gases may be deposited and cover parts of the electrodes and thus insulate parts of the surfaces, or the gases may be occluded and form an opposing gas battery. There may be an absorption of ions in the surface of the electrodes. The liberated products may form at the surface of the electrodes a new material or the same material in a different state of aggregation.

[1] F. Kohlrausch, Pogg. Ann., 148, p. 443, 1872.

[2] M. Wien, Wied. Ann., 58, p. 37, 1896.
C. M. Gordon, Wied. Ann., 61, p. 1, 1897.
A. M. Scott, Wied Ann., 67, p. 388, 1899.
E. Warburg, Wied. Ann., 67, p. 493, 1899.
E. Newmann, Wied. Ann., 67, p. 500, 1899.

The solution may act upon the electrodes either upon open or closed circuit—an action which may be normal or arise by reason of impurities, either in the electrodes or the solution. The character of the solution at the electrodes when carrying a current may be changed sufficiently to cause action. A concentration series always arises owing to the unequal rates of migration of the ions. Any of these changes give rise to a polarization. These deposits upon or in the electrodes are frequently small and a slight variation in their total quantity affects appreciably the electromotive force, when these deposits are present at the electrodes, before the current is started. Their ions are also present in the adjacent solution, and the current affects their magnitude; therefore, the concentration series in the solution is always affected. After removal of the current the electrodes and adjacent strata of the solution would tend to return to the former state of equilibrium, and the concentration series would be removed by diffusion. However the process of diffusion is a rather slow one and could only appear rapid when the concentration strata are minutely thin. If the current had suddenly been reversed, it must have aided the return to the original condition and quickly established the same phenomena as before, but at the opposite electrodes. This is what probably happens when alternating currents are used under proper conditions. This makes the phenomena reversible, and upon its reversibility depends the accuracy of the electrolytic resistance, by means of alternating currents.

The observations for electrolytic resistance by means of the direct current, unlike the observations by means of the alternating current, have never given uniform values. The resistance is always larger when the current is small. The method of measuring electrolytic resistance by direct current involves the measurement of three quantities, viz. electromotive force, E, at terminals of the electrolyte; the current, i; and the polarization, P. The electrolytic resistance, r, is then calculated by the formula $r = (E - P)/i$. The determination of E and i is easy and accurate. However, P is not usually measured with the direct current still flowing. The circuit is broken and the voltameter thrown upon an independent current. The process requires time and a residual part only of the polarization as measured. Wiedeburg has given an experimental formula[1]

$$P=\frac{P_1}{2}\left(1-E^{-\frac{B_1}{S_1}q}\right)+\frac{P_2}{2}\left(1-E^{-\frac{B_2}{S_2}q}\right)$$

where P is the total polarization. P and P' the maximum polarization at electrodes 1 and 2 respectively. s denotes surface of electrodes, B_1 and B_2 are constants depending upon the material of the electrodes, and q is the quantity of electricity which has passed.[2] Guthe has subjected the formula to an accurate experimental test. His experimental conditions made $P_1 = P_2 = P/2$, $B_1 = B_2$, $S_1 = S_2$ and the value of P so obtained, substituted in the simple formula above, gives the uniform values for electrolytic resistance. However, the actual measurement of P or an approximation to such a measurement is very desirable. As has been pointed out by several observers, the nearer the time from the opening the one circuit to the closing of the other approaches zero, the more accurate the values obtained for P, and therefore the more accurate the calculated value for r.

The object of the following experiments, is to emphasize these conditions and to show the growth of polarization upon closed and open circuit. It has therefore been necessary to employ the direct current. P has been measured between .00006 second, and .045 second after the polarizing electromotive force was removed. The latter has also been given wide ranges both in its value and in the time during which it was applied. The smallest time intervals, so far as could be learned, are ten times shorter than any previous experimenter has used.

Fig. 1 gives a diagram of the connections. The circuits were all made and broken by a swinging pendulum. The minute lateral motion of the pendulum was a troublesome factor until the steel cone pivots were drawn so tight that the pendulum ceased vibration after a few swings. The keys 1, 3 and 4 had micrometer screw attachments so that they could be adjusted accurately in the same plane as key 2. To obtain the deflection with the standard cell, B,

[1] Wiedeburg, Wied. Ann., 51, p. 2, 302.

[2] Guthe, Phys. Review, Vol. VII., p. 193.

key 1 is fastened open with a strip of glass, keys 2 and 4 are set, the six-point switch is turned to the left and the key *b* is closed by hand about the instant the pendulum is released. This was necessary since the time period from key 3 to key 4 or from key 1 to key 4 was not long enough to allow the condenser to assume the potential of the standard, owing to the large internal resistance of this cell. To obtain the total deflection from the storage cell the manipulation is the same except key 1 is used instead of key *b*. To polarize the voltameter key *a* is closed and keys 1, 2, 3, 4, 5 and 6 are set. When the pendulum is liberated, it first meets key 5 and opens the voltameter short-circuit. The detent to key 6 is overturned last of all and this again short-circuits the voltameter. By this swing of the pendulum the galvanometer will give a deflection proportional to the fall of the potential over the resistance *R* or the voltameter *V*, according as the six-point switch is turned to the left or right respectively. This fall of the potential over *V* will be the polarizing electromotive force, if keys 3 and 4 are released before key 2. It will be the polarization electromotive force if keys 3 and 4 are

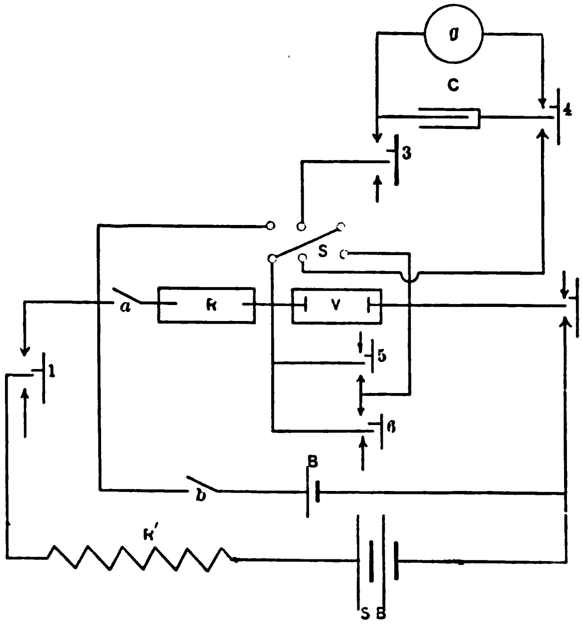

Fig. 1.

released after key 2. The deflections over R and V are also proportional to the resistance R and the apparent resistance R_v of the voltameter respectively. The deflection caused by the polarization of V, subtracted from the deflection produced by the applied electromotive force leaves a value proportional to the internal resistance r. Whenever deflections are thus compared it is necessary that the time intervals of charging be the same. For the apparent capacity of condensers vary when the time interval is very short.[1] The capacities of a one-third microfarad condenser and a one microfarad condenser were found to vary about four per cent. while a Stanley condenser varied three fold. Key 3 was placed so that it made the up contact after the detent to key 2 was released. The equation of motion of the pendulum and the distance key 4 was displaced from the plane of key 2, determines the time which has elapsed since the charging circuit was broken. To release keys 3 and 4 before key 2, a small hard rubber block was attached to the pendulum opposite keys 3 and 4. The current was determined from the total electromotive force of the storage battery and the total resistance of the circuit. When the currents were large it became necessary to measure the electromotive force of the storage cell upon closed circuit, since a concentration series[2] arises in that cell. A comparison of the deflections of the standard and storage cell gives the electromotive force of the storage cell. A deflection is then obtained from the storage when the time interval is short. Let this deflection be D'. Then, if d_r and d_v be the deflection on closed circuit over R and V when the condenser is charged the same length of time we have

$$(1) \qquad D' = \frac{(d_r + d_v)\,(R + R_r + R')}{R + R_v}$$

where D' is proportional to electromotive force of the storage cell, and R' the resistance in series with R and R_v. The internal resistance of the storage cell is neglected. Farther (2) $R_v = R\frac{d_v}{d_r}$. This value of D' differs slightly from D for large currents. However, this correction was only made in the first experiment, and for the others the electromotive force of the cell on open circuit was used.

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[1] Physical Review, Vol. IV., p. 238.

[2] Physical Review, Vol. IV., p. 353.

The adjustment of keys 2, 3 and 4 for a very short time interval was an extremely tedious task. Keys 2 and 4 were down contacts and the best method to adjust them was found to be by the sense of touch. The detents were held with the fingers against the spring contacts and the pendulum drawn back through an arc of about 2 to 5 mm., and allowed to fall against them. The time sense in the touch of the fingers is so delicate, that, by this method, a distance of .007 mm. could be detected. When the pendulum swung through its full arc the time required to pass through this distance at the lower point of the arc was .00002 second. The smallest time between keys 2 and 4 actually used was .00006 second, and in this time interval there is liable to be an error as large as twenty per cent., in a few cases. This error arises from the inaccuracy of the adjustment, temperature changes in the keys, lateral vibration in the pendulum, and any displacement of the keys due to pressure in making the settings. This setting could not be made one day and be found in adjustment the day following. Neither could it be made and key 3 adjusted to fall with its up contact between keys 2 and 4. Key 3 with an up contact had to be adjusted electrically and after much use it was found that its position would depend upon the electromotive force employed. The adjustment of keys 2, 3 and 4 for such a time interval requires three hours' work and then the adjustment could not be expected to remain over one half hour. After long trial, this short time interval was abandoned. A trial made it about .0005 second, revealed the fact that key 3 could be dispensed with so far as the polarization phenomena were concerned. The readings were the same, after allowing for difference in time interval of charging the condenser through the external resistance *R*, when key 3 was set as when it was allowed to remain against its up contact. In the latter case the condenser becomes charged to the polarizing electromotive force, and then upon open circuit discharges through the voltameter until it reaches the polarization electromotive force. This discharge would tend to maintain the polarization if the quantity is an appreciable part of the "polarization capacity" of the voltameter. Farther the electromotive force of the condenser would not have fallen to the polarization electromotive force if the time interval was sufficiently short. This time interval must be determined from the equation of discharge of a condenser $q = \left(Qe^{-\frac{t}{RC}}\right)$ or $d = \left(De^{-\frac{t}{RC}}\right)$ where D is the total deflection of a galvanometer produced by a given electromotive force. E applied to a condenser of capacity C, and d the deflection of the same galvanometer after the condenser charged to the same electromotive force has been allowed to leak through a resistance R for the time interval t. A test of this kind was applied by charging the condenser over the resistance R made approximately equal to the resistance of the voltameter, and discharging it after leaking for an interval t. It was possible in this way to adjust the keys 3 and 4 so as to obtain an appreciable deflection, d. It was, however, not possible to obtain a deflection when the time interval was as long as .00006 second, provided the electromotive force of polarization was small. When the electromotive was large the spring in .00006 second is not sufficiently far removed from the lower contact to break the primary circuit. This process therefore became an excellent independent method to use as a check upon the settings of the keys. When the condenser is charged by the polarization electromotive force, after the polarizing electromotive force is removed, it lowers the potential of the polarization electromotive force, if the charge in the condenser is an appreciable part of the total charge in the voltameter, according to the law of divided capacities. For the time periods longer than .0006 second, a difference of two per cent. could be accounted for by experimental errors, but if there had been a difference of four per cent. in the readings, it could have been stated with some assurance that the capacity of the voltameter was between twenty-five and one hundred times as great as the capacity of the condenser. No such difference was detected and key 3 was allowed to remain against the up contact for all the recorded measurements and was only set when it was desired to make a test for capacity. No such effect was ever found with certainty but it seems reasonable to anticipate capacity measurements when the surface of the electrodes is reduced considerable in magnitude. After omitting the setting of key 3 the adjustment of keys 2 and 4 was greatly facilitated, and it became again possible to set to .00006 second, when the electromotive force was small. Wires not shown in the figure also lead from the voltameter to an alternating current bridge where the electrolytic resistance was determined by the Kohlrausch method.

The density of the solution used was 1.1395 throughout the experiments. The evaporation from the surface was compensated by adding distilled water. The temperature varied about four degrees in the first experiment, but less in the others. All solutions, except the first, contained about one-tenth per cent. of alcohol. The object of the alcohol was to prevent oxidation of the electrodes, but in this proportion it was not a success. The solution was also boiled to free it of the contained oxygen. Fresh solution from a stock bottle was taken for each experiment. The results of the first experiment are contained in Tables I. and II., and Figures 2, 3 and 4. The copper plates had stood in the solution for an entire week during preliminary tests, and the experiment itself continued through another week. At the end of this interval the plates were

TABLE I.

i.	E.	R_r	i.	E.	R_r
.00188	.106	56.44	.0274	.701	25.6
.003575	.236	66.	.0506	.9973	19.71
.00906	.435	48.	.0817	1.147	14.04
.01289	.5143	39.9	.1097	1.234	12.5
.01969	.6773	34.4	.2118	1.3765	6.5

TABLE II.

T	$E = .236$		$E = .4349$		$E = .701$		$E = 1.147$	
	P	r	P	r	P	r	P	r
.00006			.2176	24.	.3570	12.6		
.00018	.0898	40.6	.136	33.			.2973	10.4
.000295					.2352	17.		
.00033	.0512	5.7	.0671	40.6				
.00053			.0634	41.	.1753		.1911	11.7
.00073			.0544	42.		19.2		
.00090	.285	58.						
.00100			.0364	44.				
.00680	.0215	60.	.0309	44.6	.1366	20.6	.1666	12.
.01400	.018	61.	.0245	45.3			.1503	12.2
.0800			.0182	46.				

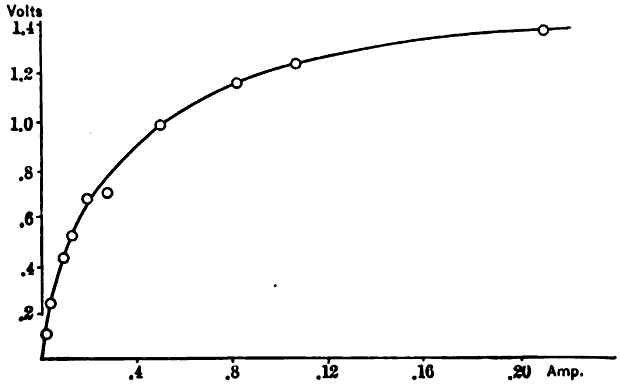

Fig. 2.

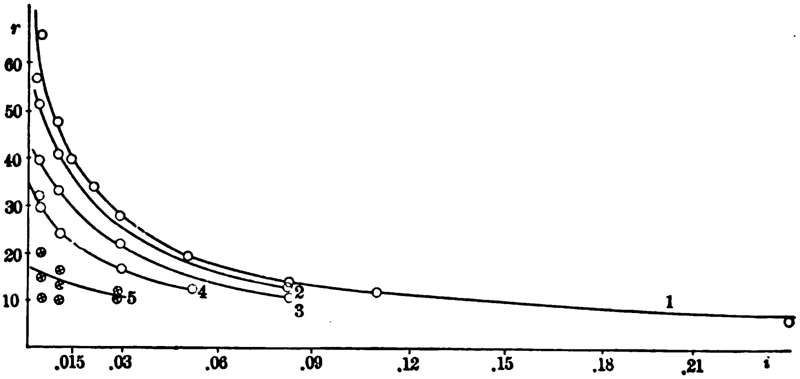

Fig 3

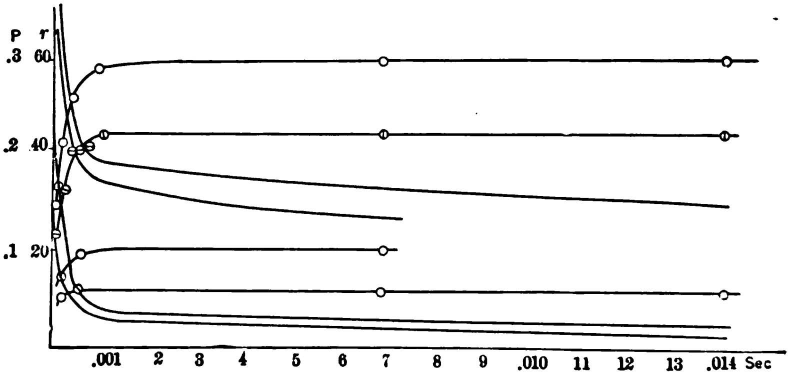

Fig. 4.

colored a dark green, but the uniformity of the readings indicate that the plates did not change throughout the experiment. The test of the resistance by the Kohlrausch method gave a value of thirty-three ohms, but the capacity effect was so large that the adjustment could not be made closer than about five per cent. The measurement of resistance by direct current indicated values much smaller than this. Later it appears that the direct current flowing only for a brief interval had an apparent temporary cleaning action upon the electrodes. The old plates were then thoroughly cleaned and immersed in the same vessel in another solution from the same stock bottle. The resistance now obtained by Kohlrausch's method was 3.9 ohms. The areas of each of these plates was 60 sq. cm. The primary circuit was closed approximately .08 of a second. Fig. 2 shows the relation of the polarizing electromotive force to the current. The ratio of these two quantities is the apparent resistance. In Fig. 3 resistances are plotted as ordinates and currents as abscissæ. The upper curve gives the apparent resistances. The other curves give resistances after allowing for the polarization measured after regular time intervals. The curve nearest the apparent resistance shows the resistance obtained when the polarization is measured, .00033 second after the removal of the polarizing electromotive force. This is the usual character of the curve obtained by the various experimenters, though the time period usually is several times longer than .00033 second. As this time interval is shortened, the resistances diminish and show less variation with changes of current. Some observations are shown on this curve for an unknown but very short time period, the data for which are not recorded. For more minute time period one could expect the curve for resistance to run parallel with the axis of abscissæ, *i. e.*, the resistance would be the same for all currents and probably the same as the electrolytic resistance. Fig. 4 gives the relation of polarization and of resistance to time for four different electromotive forces. As is apparent, the great changes all occur in the first one-thousandth of a second. There are also greater changes for small than for large electromotive forces.

Table III. gives some results obtained with small copper plates with one-tenth the area of the previous plates. The electrolytic resistance of the solution by the Kohlrausch method was 8.5 ohms when the plates were clean and 13.6 ± 0.4 when they had stood in the solution for some time. In this experiment the plates had stood in the solution twenty-four hours. The results are shown graphically in curves *a* and *b*, Fig. 5.

TABLE III.

$T \times 10^6$	$R_0 = 73.5$ $i = .00186$ $E = .1367$		$R_0 = 51.4$ $i = .00535$ $E = .3576$	
	P	r	P	r
12	.1098	13.36	.209	12.36
30	.1049	17.10	.200	14.02
60	.1004	19.51	.190	15.86
180	.0955	22.14	.1825	17.91

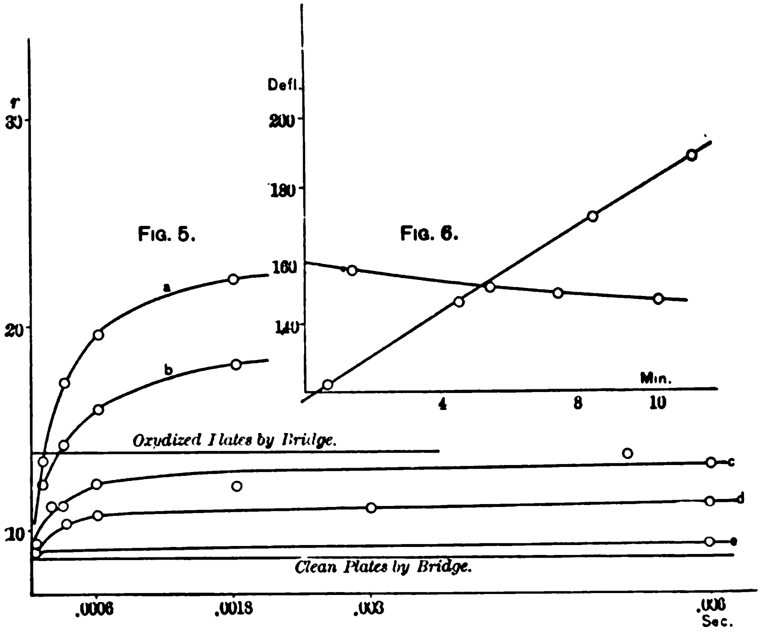

FIG. 5.

FIG. 6.

Tables IV. and V. give results for plates of the same size as in Table III. The data, however, are for plates when first placed in the solution. Oxidation occurs so rapidly that it was impossible at first to get any consistent results. The resistance increased continually as indicated both by the potential and the bridge methods. In twenty minutes the plates would be appreciably colored and had to be recleaned. The fresh prepared plates would give new values so different from the others that the difference could not be accounted for upon the ground of experimental error. Measurements were then taken at stated intervals and when these were exterpolated to zero time they became acceptably uniform. This indicated that the plates, if always cleaned with care and by the same method, would give almost the same resistance when first placed in the solution. The effects of oxidation increased the resistance fairly uniformly from this instant, but the effects were no two times alike and hence the above irregularities. It therefore became necessary to clean the plates for each determination. The circuits were then adjusted for the measurement of polarization, the plates dropped into the solution and about two seconds later the pendulum allowed to swing. The adjustments were then

Table IV.

R_v	i	E
16.11	.00050	.0081
15.2	.00193	.0283
13.12	.00454	.0596
12.55	.00587	.0737
12.6	.04580	.6771
11.	.08980	.9878

Table V.

	$R_v = 15.42$ $i = .00193$ $E = .0298$		$R_v = 12.71$ $i = .00587$ $E = .0745$		$R_v = 11.$ $i = .0898$ $E = .9878$	
	P	r	P	r	P	r
8	.0119	9.26	.0217	9.02		
18	.0083	11.11				8.8
30	.0083	11.11	.0147	10.21		
60	.0062	12.22	.0123	10.61		
180	.0064	12.1				
300			.0102	10.98		
600	.0054	13.15	.0087	11.22		9.2

made for drop over voltameter, *V*, or over resistance, *R* of 30 ohms in series with *V*, and after a definite time interval the pendulum was again dropped. An example of the observations for fall of potential over *V* and *R* is given in Table VI. and shown graphically in Fig. 6. The intercept of these curves on the *y* axis is taken for the true drop over *R* and *V*. The plates were then recleaned and the keys set for another period of polarization and observations repeated. Any slight variation in voltameter resistance gives a corresponding variation in applied potential, polarization and current. The mean of these values is recorded in Table IV. A more complete set of observations than given in Table V. is desirable, but, owing to necessity of cleaning the plates, to obtain such a set is an extremely tedious task. The data for resistance in Table V. are shown graphically in curves *c*, *d* and *e* of Fig. 5. Curves *a* and *b* arose from polarizing electromotive forces lying between the potentials causing the polarization *d* and *e* and that is where *a* and *b* should be expected to fall if the plates used in Table III. had been clean instead of being oxidized. We may therefore say that a large part of the effect in *a* and *b* arises from a change in the condition of the surface of the electrodes and consequent changes in adjacent solution and that the effect in *c*, *d* and *e* arises largely from the true concentration series.

TABLE VI.

T min.	Drop over 30 ohms.	Drop over *V*.
.75		122.2
1.5	27.6	147.
4.5		
5.5	270.	
7.5	268.4	
8.5		171.6
10.5	267.6	
11.5		189.

In the next experiment the polarizing electromotive force varied from .0003 second to .1637 second. The copper plates were the same as in the previous experiment. The resistance external to voltameter was constant. As the time of the applied electromotive force increased the polarization also increased and there was, therefore, an increase in the terminal electromotive force. This occasions an increase in the apparent resistance of the voltameter and a corresponding decrease in the current. These results are shown in Table VII. For each polarizing period the polarization was measured at different intervals. The results obtained are given in Table VIII. Fig. 7 shows the results graphically. T_v represents the time the voltameter was polarized and T_p the time elapsing between breaking the circuit and measuring of the polarization. The ordinates represent electromotive forces for both sets of curves. The curves with ordinates increasing with abscissæ each correspond to a constant T_p and variable T_v. For the remaining curves T_v is constant and T_p variable. The abscissæ are nine times larger for variable T_p than for variable T_v. The former show the decrease of polarization when the polarizing electromotive force is removed; the latter show the increase of polarization when the current is applied to the voltameter.

Table VII.

T	i	R_v	E
.0003	.001954	16.84	.0329
.0030	.001927	23.12	.0430
0240	.001914	38.10	.0729
0634	.001895	48.64	.0923
.1637	.001885	54.13	.1010

Table VIII.

T_p	$T_v = .0003$		$T_v = .003$		$T_v = .024$		$T_v = .0634$		$T_v = .1637$	
	P	r	P	r	P	r	P	r	P	r
.00012	.0077	12.90	.0181	13.71	.0367	18.94	.0529	20.44	.0725	15.66
.00030	.0067	13.24	.0152	15.21	.0302	22.32	.0453	24.46	.0611	21.71
.00060	.0058	13.89	.0123	16.72	.0274	23.81	.0425	25.96		
.00120	.0058	13.89	.0093	18.28	.0257	24.71	.0414	26.51	.0575	23.63
.00300	.0058	13.89	.0078	19.09	.0257	24.71	.0409	26.78	.0518	25.56
.00600	.0056	14.02			.0221	26.60	.0388	27.88	.0518	26.66
.01800	.0043	14.69	.0075	19.54	.0166	29.49	.0300	32.52	.0404	32.70

r by alternating current = 13.6.

Fig. 8 shows the value of r as ordinates and T_p as abscissæ. They show no regularity as to each other. With more accurate measurements they possibly would be found to intersect the y axis at the point corresponding to the electrolytic resistance.

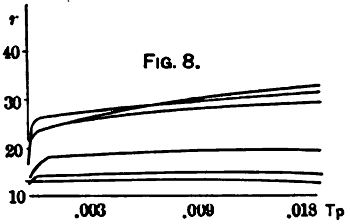
Fig. 8.

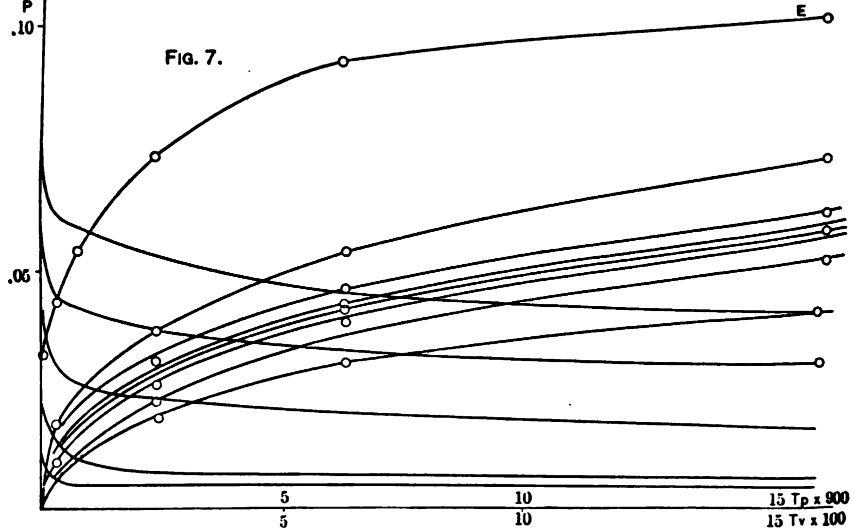
Fig. 7.

These observations show clearly the rapid formation of the polarization and that polarization arises for even minute current thrusts and for alternating currents. At first the current produces a change at the contact of solution and electrodes. It then effects deeper layers of the solution and the effect at the contacts of electrodes and solution increases, but less rapidly. This gives rise to further, but smaller increases in polarization. Diffusion diminishes the increase in polarization and after a time there is a constant potential between the terminals of the voltameter or equilibrium between the diffusion process and the increase in potential. On open circuit the diffusion still continues and affects most rapidly the strata nearest the electrodes were in many cases a reaction occurs. Therefore at the electrode surfaces the changes are greatest. When all the strata affected by the concentration changes are extremely thin, then the polarization will decrease very rapidly. This effect is shown in the polarization curves and the rapid changes of the apparent resistance when the applied current was very small.

When the currents become larger there are concentration changes farther from the electrodes, and the greater the distance from the electrodes to the outlying concentration changes the less P will diminish in a short interval. Therefore, the values obtained for the true resistance when large currents are used, provided they are not so large as to complicate the phenomena, correspond more nearly to values obtained with alternating currents. Since the circuits were not closed long enough to establish a maximum of polarization, one might anticipate that the same effect would be produced by increasing the length of time, the polarizing electromotive force was applied. In Table VIII. it is seen the polarization does increase as T_v increases and that the residual polarization is more persistent on open circuit for the larger values of T_v. However, when the current is applied for a longer time the curve for apparent resistance does not become more nearly horizontal but becomes more bent. This would indicate that as the polarizing electromotive force increases and the current decreases the part of the polarization at the contact of the electrode and the solution increases more rapidly than the part farther removed from the electrodes. Therefore, there is a greater change in polarization for about the same current, when the time for the polarizing electromotive force is increased. This point, however, needs further investigation. The intercept on the y axis of the E curve divided by the current gives the electrolytic resistance as measured by the alternating current. The polarization curves with varying T_v all pass through the origin. The value of the polarization T_v for $T_p = 0$ would be difficult to exterpolate. These polarization curves approach nearer the E curve as time increases and makes r apparently increase and later decrease with increasing time. This may be due to some inaccuracy in the measurements, but it is not altogether impossible since the polarization is very persistent when the time is longer as indicated by the curves where T_p is variable. The curves plotted with the last factor as variable indicate that there is a considerable part of the polarization very persistent. There is a part also which diminishes very rapidly, and which, by ordinary methods, has not been observed. The former probably concerns the strata of the concentration series more remote from the electrodes. The latter probably arises from the outer strata of the concentration series adjoining the electrode, where in the case of the oxidized plates reaction at once sets in. In case of clean copper plates this action would be scarcely noticeable, and therefore the slight variation of the *c*, *d* and *e* curves, Fig. 5. The curves and the data also indicate that if the true maximum value of *P* could be measured, the simple formula in ordinary use would give nearly the true resistance of the cell for all currents. That it should give the true electrolytic resistance when the measurements are very accurately measured should not be expected under all conditions. For concentration changes mean layers of different conductivity from the body of the solution. The heating of the electrodes may not be altogether negligible, particularly if either electrode is small and current density large. Gaseous products, and a change in the aggregation of the electrode surface also produce changes, since the current must either pass around or through them. Such phenomena as these together with the unmeasured part of the polarization, constitute what has been designated as a "transition resistance." It is always desirable to have as few complications as possible.

The time periods here used are so short one can estimate the polarization which would arise when an alternating current of slow frequency, like that used in electrolytic measurements, is applied. Let us assume a current alternating five hundred times to the second, having the same current strength as used in this experiment. Assume the average electromotive force to be seven-tenths of the maximum. Then the maximum would be reached in .001 second and the polarization would therefore be about .01 volt.

It is the author's intention to farther apply this method to thoroughly oxidized plates, and to clean electrodes, if oxidation of the latter can be prevented. Formaldehyde is recommended by some to prevent oxidation and will be tried. Farther tests for capacity; and for other solutions and other electrodes are desirable. To diminish the time period much more will not be possible since the shortest time already is near the limit of the time for the condenser to acquire full charge. However, a change in the apparatus so as to obtain greater accuracy in the measurement of the time is very desirable. The method is also applicable to measurement of internal resistance and polarization in primary batteries.

University of Nebraska, June, 1899.

A NEW METHOD OF COMPARING TWO SELF-INDUCTANCES.

HUBERT V. CARPENTER.

DURING a careful study of the more recent methods of measuring and comparing inductance and capacity I have developed the following method of comparing two self-inductances which gives very accurate results. Its freedom from the effects of other inductances permits its use for the comparison of very small values, and of values which are very different.

If l_1 and l_2 are the two coils whose coefficients of self-inductance, L_1 and L_2, are to be compared, connect the two in parallel having in series with one of them a variable non-inductive resistance. Apply to their common terminals an harmonic electromotive force. This will cause currents to flow through the coils which will lag behind the impressed electromotive force by the phase angles:

$$\varphi_1 = \tan^{-1}\frac{\omega L_1}{R_1}, \text{ and } \varphi_2 = \tan^{-1}\frac{\omega L_2}{R_2},$$

in which R_1 and R_2 are the total resistances of the two parallel branches including the coils l_1 and l_2, the variable resistance in one branch, and the connecting wires; while ω is equal to 2π times the frequency of the alternating current or $2\pi n$. This shows that when the two ratios $\frac{\omega L_1}{R_1}$ and $\frac{\omega L_2}{R_2}$ are equal the two currents in the parallel branch will be in the same phase. If, then, we can by any means produce and detect this condition of phase equality, we get at once the relation between the two inductances. It is simply, $\frac{\omega L_1}{R_1} = \frac{\omega L_2}{R_2}$, or $\frac{L_1}{L_2} = \frac{R_1}{R_2}$.

The two primary currents can be brought into phase equality by varying the non-inductive resistance in one of the branches. This condition can be detected in the following manner: Introduce a secondary circuit which is made up of two equal coils, s_1 and s_2, of many turns, and a telephone, all connected in series. Place coil s_1 close and parallel to l_1, coil s_2 close to l_2. The currents in l_1 and l_2 then induce electromotive forces in s_1 and s_2 respectively. The geometric sum of these two secondary electromotive forces will cause a current through the telephone. Suppose, however, the coils s_1 and s_2 to be so connected that when the currents in l_1 and l_2 are in phase with each other, the induced electromotive forces, Es_1 and Es_2 will be opposed to each other in the secondary circuit. The resultant secondary electromotive force, Es, will then be equal to $Es_1 - Es_2$; or, if these be made equal, will be zero. This condition will be detected by the silence of the telephone.

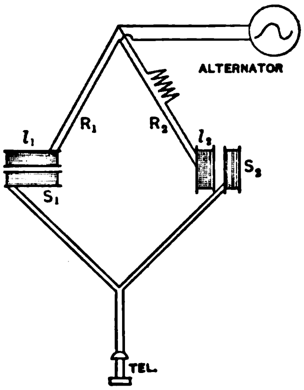

Fig. 1.

This silence indicates two things: 1st, that the two primary currents, Cl_1 and Cl_2, are in the same phase; 2d, that $Es_1 = Es_2$. Since the first condition is true, when $\frac{\omega L_1}{R_1} = \frac{\omega L_2}{R_2}$, it can be made true by varying R_1 or R_2. The second condition may be secured by varying the relative position of one of the secondary coils, S_2 for instance, with respect to its primary. This will change the electromotive force induced in it, and if it be moved back and forth until a position is found where the hum in the telephone is a minimum or zero we know that it is so placed that $Es_1 = Es_2$. If this adjustment does not give silence but only a minimum sound in the telephone it shows us that the two electromotive forces, Es_1 and Es_2, are not in exact opposition. This means that the two primary currents, Cl_1 and Cl_2, are not in the same phase and so R_1 or R_2 must be changed. After the value of R_1 or R_2, and the position of S_2 have been adjusted so as to give silence in the receiver, it is only necessary to know $\frac{R_1}{R_2}$ to get $\frac{L_1}{L_2}$.

The proper position of S_2 can be found so quickly that the work of obtaining a balance is but little more tedious than in the Wheatstone's bridge. When using the telephone as a current detector, however, there is no means of knowing which way R_1 or R_2 must be varied to reduce the phase difference between Cl_1 and Cl_2. This corresponds to the same weakness of the telephone when used in the Wheatstone's bridge.

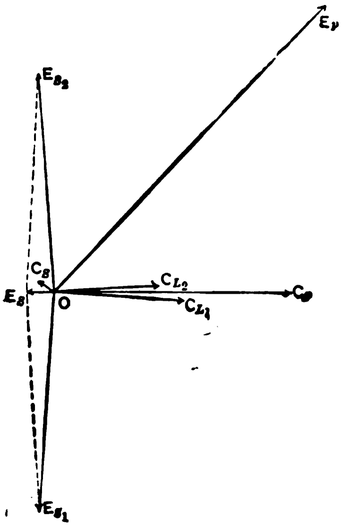

Fig. 2.

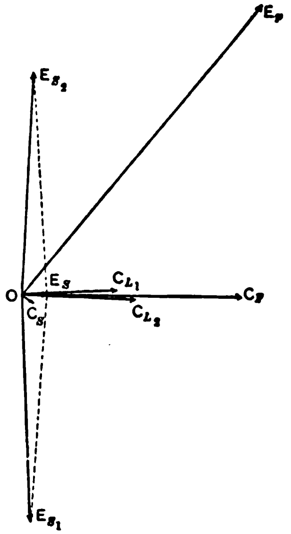

Fig. 3.

The phase relations of the currents and electromotive forces just discussed may be seen upon the two phase diagrams, Fig. 2 and Fig. 3. In these diagrams the vectors represent by their length the maximum values of the currents and electromotive forces, while their directions indicate their respective phases. The instantaneous values of all the quantities at any instant is given by the lengths of the projections of their respective vectors upon a moving vector which rotates in the clockwise direction through the entire 360° with each cycle of the current. According to this the secondary electromotive force, Es_1, reaches its maximum one quarter of a cycle later than the current Cl_1 which induces it, as it should. The vector Es_2 is shown reversed or 180° out of its true position. This corresponds to the physical condition of being connected in series with Es_1 in such a way that they oppose each other. As may be seen from the two diagrams, the resultant secondary electromotive force may have any phase whatever and varies with every change in the value of R_1 or R_2, or in the position of the coil S_2.

Careful tests were made of the method in this form. With a fairly good telephone, whose resistance was much too low for the greatest sensitiveness, successive comparisons could be made to agree within one-half of 1 per cent. without difficulty.

In order that these results may not only agree among themselves, but may be correct, the apparatus must be so arranged that there shall be no appreciable inductive effects except those which are accounted for. To do this the two coils under comparison, l_1 and l_2, which carry fairly large currents, must be so placed as to prevent mutual inductance between them. This was accomplished by placing them about two meters apart and in mutually perpendicular planes. Tests were made which showed this to be effectual. The lead wires from the source of the primary current to the point where the circuit divided were made up of twisted lamp cord and were kept as far as possible from the coils. The leads to each coil from this dividing point were placed about one centimeter apart and crossed over at frequent intervals. In comparing coils of low inductance it would be best to use lamp cord here also, as the effect of capacity would be very small compared with the inductive effect of the large current.

Any current which flows in the secondary circuit will, of course, react upon the primary currents, but when a balance is reached the secondary current disappears, and with it its disturbing influence. The secondary electromotive forces remain, however, and, together with the distributed capacity in the secondary coils, will set up a small local current in each coil. The amount of this hidden current is hard to estimate. It depends upon the capacity of the coil, which we can do no more than estimate, and its value is different at every point along the conductor. It can easily be shown to be very small however. Two pairs of coils were used at different times as secondaries, one pair of 300 turns each of No. 25 B. & S. wire and about 30 cm. in diameter; the other pair of 3000 turns each of No. 30 wire and about 10 cm. in diameter. The latter were made up of 50 layers of 60 turns each. If we were to consider each layer as a plate of a condenser with alternate plates connected together the calculated capacity would be about .012 microfarad. In the coil, however, the difference in potential between layers is only about one-fiftieth of the potential across the terminals of the coil so the effective capacity will be something like .0002 microfarad. Suppose the total electromotive force induced in the coil to be as great as 50 volts or one volt per layer, and assume that the resistance and self-induction of the coil does not prevent it from becoming fully charged at each impulse of the current. Then, if the current in the primary passes through sixty cycles per second the local current in each secondary coil will be :

$$I = \frac{Q}{t} = \frac{EC}{t} = 120 \times 1. \times .0000000002 \text{ or about } 24. \times 10^{-9}$$

ampères. If the coils of 300 turns each were used the local current would be much less than this. If this estimate be correct it shows that the reactive effect of these local currents upon the primary coils is entirely negligible. Further, it is easily seen that both primary coils are affected in the same way so that if the primaries were equal the error would be zero. Tests were made with both pairs of secondary coils upon the same primaries and the fact that no difference in the results due to the change if secondaries could be detected gives experimental evidence that the effect of the local currents is not measurable.

The next step in the development of the method was to replace the telephone by the electro-dynamometer. This required considerable change in details, but the principle remains the same, and the results showed a marked improvement in accuracy. The ordinary dynamometer having both coils connected in series is not sensitive to small currents, so a special instrument was used, in which the fixed coil was made up of 600 turns of No. 25 wire, and the suspended coil of 380 turns of No. 36. The latter was suspended by a phosphor bronze strip of about three circular mils cross-section and ten inches in length. This suspension strip and a similar strip in a loose spiral below the coil formed the connecting wires to the suspended coil. These strips would carry about .01 ampère for a short time, while the fixed coils of the instrument would safely carry 1,000 times as great a current for the same length of time. No metal was used in the instrument near the coils except the conductors themselves, because it was designed for use in testing Professor Rowland's phase angle methods. In the new method it is used only to detect small currents, and for this could be improved by adding more turns to the suspended coil and an iron core to the fixed coils.

The small secondary current, which is to be made zero, is passed through the suspended coil, and the heavy primary current through the fixed coils of the dynamometer. In this way the instrument was made sensitive to currents of about $10.^{-8}$ ampères. The directian of the deflection of the suspended coil reverses when the phase of the current in either coil is changed by 180°, so the instrument will give an indication concerning which way the resistances of the primary branches must be changed to bring the balance nearer. This is another advantage over the telephone. One peculiarity of the dynamometer must be remembered, however. If the two currents in the two coils differ by 90° in phase there will be no deflection. It becomes necessary, then, to distinguish between a zero dedeflection due to zero secondary current and a zero deflection due to 90° phase difference between the primary and secondary currents. The phase relations between these two currents may be seen in the phase diagrams, Figs. 2 and 3. We see that without some other arrangement it is impossible to tell whether the dynamometer is indicating zero secondary current or 90° phase difference. This difficulty was met in the following way: A non-inductive shunt, R_n, was connected across the terminals of the fixed coils of the dynamometer, with a key in its circuit so that it could be cut out at will. When this is connected the main primary current is divided between the two paths, and since the coils have considerable inductance while the shunt has not, the current in the fixed coils will lag behind the total primary current by a considerable angle. The currents and electromotive forces of the coils, l_1, l_2, s_1, s_2, do not change their relative phases in any way when this shunt connection is made, so the result is that the difference in phase between the two dynamometer currents is altered. If it was 90° before the shunt was connected, it must be something more or less afterwards, and unless the secondary current is zero, there will be a deflection of the suspended coil. This result could have been attained easily in other ways, but this seemed the simplest and proved very satisfactory.

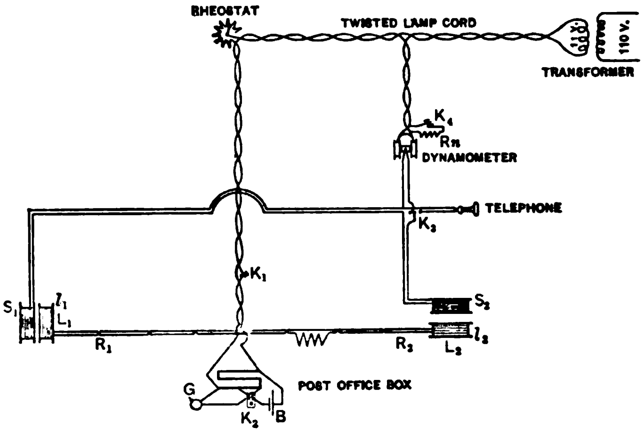

Fig. 4.

The connections as finally used are shown in the large diagram, Fig. 4.

The theory so far given is dependent upon the assumption that the electromotive force impressed upon the primary circuit is an harmonic function of the time. Since, however, the ratio of inductance to resistance in the two branches of the primary circuit are made the same at the final balance, it is true that, no matter what electromotive force is applied to their common terminals the currents in the two branches will rise and fall together. The two secondary electromotive forces will then have the same wave form and phase, and can be made to neutralize each other completely. This was shown to be true experimentally by the fact that when a balance had been obtained by the use of the dynamometer and an alternating current, no sound would be heard in the telephone when an interrupted current was used. This extreme test is certainly ample verification. The question of the source of the electromotive force is now an easy one, for any alternating current of any frequency, wave form, or degree of unsteadiness may be used. The tests given herewith were made with current from a two-phase, 60-cycle, Westinghouse generator. No tests of the wave form were made but the makers state that it differs from a sine curve by less than five per cent. at every point.

Let us consider the conditions under which the method will give the best results. In short, what is the relation between L_1 and R_1, or between L_2 and R_2, for maximum sensitiveness? The resultant electromotive force in the secondary circuit, E_s, also the secondary current, C_s, upon which the observations depend, varies, for small differences, with the angle of phase difference between Cl_1 and Cl_2. For maximum sensitiveness a given small change in the value of R_1 or R_2 should produce a maximum change in this angle, θ, of phase difference; or, the rate of change of R_1 with respect to θ should be a minimum. θ is equal to $\varphi_1 - \varphi_2$, where φ_1 and φ_2 are the phase angles by which the currents Cl_1 and Cl_2 lag behind the primary electromotive force, and to produce a change in θ we change either φ_1 or φ_2 while the other is unaltered. It follows then that a given change in φ_1 will produce an equal change in θ. This being true we can say that, for maximum sensitiveness, the rate of change of R_1 with respect to φ_1 must be a minimum.

The relation between R_1 and φ_1 is,

$$\tan \varphi_1 = \frac{\omega L_1}{R_1}$$

or,

$$R_1 = \omega L_1 \cot \varphi_1$$

The rate of change of R_1 with respect to φ_1 is then:

$$\frac{dR_1}{d\varphi_1} = - \omega L_1 csc^2 \varphi_1.$$

This rate will be a minimum when:

$$\frac{d^2 R_1}{d\varphi_1^2} = 0, \text{ or when } 2\omega L \cot \varphi_1 csc^2 \varphi_1 = 0.$$

Substituting for the trigonometric functions their values in terms of ωL_1 and R_1 we have,

$$2 \cdot \omega L_1 \cdot \frac{R_1}{\omega L_1} \cdot \frac{R_1^2 + \omega^2 L_1^2}{\omega^2 L_1^2} = 0.$$

Since R_1 is the only variable included in the expression we see that the most accurate results will be attainable when the resistances R_1 and R_2 are as near zero as possible. In other words, the greater the angle by which the primary currents lag behind the electromotive force the greater will be the accuracy of the results. The formulæ also show that the sensitiveness when the primary currents lag 45° is one-half of the theoretical maximum. This is attainable in the case of most ordinary coils with a frequency of sixty.

As may be seen from Fig. 4 the telephone and dynamometer were connected so that either could be used in the secondary circuit. In practice the telephone was used in the preliminary adjustments and the dynamometer for obtaining the balance. The postoffice box was connected to the central posts where the leads to the primary coils were joined and as soon as an adjustment was completed a lead wire of one coil, say l_1, was disconnected and the resistance of l_2 measured; then l_2 was disconnected and l_1 reconnected and its resistance measured. In this way R_1 and R_2 were determined before any appreciable change could occur for care was always taken to avoid heating the coils. The postoffice box used was the best in the laboratory and can be depended upon to give results within $\frac{1}{10}$ % of the correct values while the ratios given by it are considerably more accurate because the two resistances were measured under the same conditions and as far as possible the same coils in the box were used. The resistances given below have all been corrected for the resistance of the leads from the postoffice box to the common terminals of the coils.

The inductances of five different coils were compared. The coils will be designated, l_1, l_2, l_3, l_4, and l_5, with inductances, L_1, L_2, L_3, L_4 and L_5, respectively. Coils l_1 and l_2 were wound in regular layers in a rectangular groove cut in a disk of plaster of paris, and their dimensions accurately determined so that their inductances could be calculated. Coil l_1 was made up of 70 turns in 7 layers of No. 20, B. & S. gauge, single silk-covered copper wire. Its mean diameter was 38.605 cm. Calculated inductance, $L_1 = .0049046$ henry.

Coil l_2 was made up of 8 turns in 2 layers of No. 15 B. & S gauge, double cotton-covered copper wire. Its mean diameter was 38.681 cm. Calculated inductance, $L_2 = .0000726$ henry. These inductances were calculated by the formula due to Maxwell and Stefan.[1] The value calculated for L_1 was assumed to be correct and made the basis for finding the values of the others from the ratios. Coils l_3, l_4 and l_5 were of unknown dimensions and wound on wooden frames.

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[1] Stefan, Wied. Ann., V. 22, p. 107, Apr., 1884.

DATA. TABLE I.

COMPARISON OF COILS l_1 AND l_3.

R_1	R_3	L_3	
3.1574	27.865	.043285	
3.1198	27.559	.043324	
3.1104	27.479	.043327	
3.0940	27.363	.043375	
3.0690	27.115	.043330	
		Average =	.043328
		Probable error = .000010 =	.02 + per cent.

TABLE II.

COMPARISON OF COILS l_1, AND l_4.

R	R_4	L_4	
2.9785	30.828	.050762	
2.9900	30.985	.050825	
3.0212	31.209	.050662	
3.0380	31.459	.050790	
2.9911	30.978	.050793	
2.9068	30.038	.050682	
2.9542	30.594	.050790	
2.9357	30.427	.050830	
		Average =	.050767
		Probable error = .000014 =	.03 — per cent.

TABLE III.

COMPARISON OF COILS l_3 AND l_4.

R_3	R_4	L_4	
26.855	31.450	.050741	$L_4 = \frac{R_4}{R_3} L_3$ where $L_3 = .043328$
26.905	31.547	.050802	
26.897	31.540	.050808	
26.873	31.427	.050665	
26.800	31.406	.050774	
27.023	31.647	.050740	
26.915	31.513	.050730	
26.950	31.520	.050675	
		Average =	.0507445
		Probable error = .000012 =	.02 + per cent.

Table IV.

COMPARISON OF COILS l_1 AND l_5.

R_1	R_5	L_5	
3.1641	.26870	.00041650	
3.1800	.26911	.00041510	
3.1905	.27033	.00041555	
3.1847	.27009	.00041593	
3.1838	.26996	.00041585	
3.1775	.27013	.00041695	
3.2108	.27266	.00041650	
		Average =	.00041605
	Probable error = .00000016	=	.04 — per cent.

Table V.

COMPARISON OF COILS l_2 AND l_5.

R_5	R_2	L_2	
.76554	.13177	.000071615	
.76513	.13182	.000071680 ·	
.76579	.13185	.000071628	
		Average =	.000071641

The values given for L_2 in Table V. are based on the value of L_5 found in Table IV. This depends upon the calculated value of L_1, so that in the approximate agreement between the above value and the calculated value of L_2 we have an inaccurate check upon the correctness of both. Table III. gives a direct check upon the correctness of the ratios $\frac{L_1}{L_3}$, $\frac{L_1}{L_4}$ and $\frac{L_3}{L_4}$. From the data there given the ratio of L_3 to L_4 is obtained, and the values of L_4 given are based upon the value of L_3 obtained in Table I. The two values L_4 differ by 0.05%, but this represents the combined error in three independent sets of observations.

The method here outlined might also be applied to the comparison of two capacities by connecting one in series with each primary coil l_1 and l_2. The results, however, would require correction for the inductances L_1 and L_2, and, further, unless the two condensers were exactly similar in construction, the result would be in error, because of the unequal rates of absorption.

Physical Laboratory, University of Illinois, April, 1899.

NEW BOOKS.

Lehrbuch der Experimentalphysik. Vierter Band; *Die Lehre von der Strahlung.* By ADOLPH WÜLLNER. Leipzig: B. J. Teubner, 1899.

The fourth volume of the fifth edition of this standard treatise deals with radiation. Its new title differs from that of the corresponding volume of the earlier editions, which was entitled *Die Lehre vom Lichte.* The work has grown in the attempt to incorporate the results of the most important newer investigations from 624 pages in the third edition of 1875, to 1,042 pages in the edition of 1899.

This volume shows indeed a greater amount of new material than any of the other volumes of this edition, and while the plan of the work remains unchanged, one is pleased to recognize the introduction of full and very satisfactory descriptions of such investigations as that of Wiener, on "Standing Light Waves," of Kerr on "Electro-optic Double Refraction," of Drude and of Rubens on "Metallic Refraction," etc.

The most important portions of Zeemann's work on the influence of "Magnetization and the Emission of Light," appeared too late to find a place in this edition, but his earlier papers of 1897 are briefly described.

E. L. N.

Ostwald's *Klassiker der Exakten Wissenschaften.* No. 99, *Ueber die Bewegende Kraft der Wärme,* by R. CLAUSIUS. No. 100, *Abhandlungen über Emission und Absorption,* by G. KIRCHHOFF. Edited by Professor MAX PLANCK. Leipzig: Wilhelm Englemann, 1898.

With these two volumes, this well-known series of German scientific-reprints reaches its 100th number. Of the two memoirs selected by the editor, Professor Planck, for reprinting, the papers of Kirchhoff were perhaps more in need of being brought into convenient and accessible form than the well-known work of Clausius, on the "Motive Power of Heat." Both indeed are easily accessible in German, the latter having been reproduced in the first edition of Clausius's treatise while Kirch' hoff's memoirs are to be found not only in the Transactions of the Berlin Academy of Science, a set which is not always at one's disposal, but likewise in his volume of *Gesammelten Abhandlungen.* Of Clausius paper there exist at least three literal English versions. The value to science in these reprints will be found, therefore, rather in the two brief notes of the editor than in the reproduction of the texts themselves.

E. L. N.

Inorganic Chemical Preparations. By FELIX LENGFELD. Pp. xvii+55. New York, The Macmillan Co., 1899. (*Received.*)

Laboratory Manual. Experiments to illustrate the Elementary Principles of Chemistry. By H. W. HILLYER. Pp. vi+200. New York, The Macmillan Co., 1899. (*Received.*)

Volume X. *February, 1900.* *Number 2.*

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THE
PHYSICAL REVIEW.

THE INFLUENCE OF ELECTRIFICATION UPON THE SURFACE TENSION OF WATER AND MERCURY.[1]

By Ernest Merritt and Samuel J. Barnett.

THE investigations of Exner and Tuma,[2] Warburg,[3] G. Meyer,[4] Luggin,[5] S. Meyer,[6] Nernst,[7] Behn,[8] S. W. J. Smith,[9] and many others have established beyond reasonable doubt that the electrocapillary phenomena occuring at the interface between a liquid metal and an electrolyte, in a capillary electrometer, for example, are due almost entirely to true alterations of surface tension resulting from the chemical changes of electrolysis. There are many other electrocapillary phenomena, however, which are undoubtedly caused largely, if not wholly, by the electrostatic forces that are developed by the presence of a charge upon the liquid surface. Thus when electrified, a soap bubble expands, a globule of mercury becomes more flattened, the wave pattern of a liquid jet issuing from an elliptical aperture becomes more elongated,[10] etc., etc. In such cases the effect of electrification is usually similar to that which would be produced by a *diminution* of the surface tension. This fact is probably due to the universal employment of convex surfaces in such experiments; in the case of a concave charged surface a consideration of the electrostatic forces would lead us to expect an apparent increase in the strength of the surface film.

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[1] A paper read before the American Physical Society, December 28, 1899.

[2] Wien. Ber. 97 IIa, p. 923, 1888.

[3] Wied. Ann. 38, p. 321, 1889; ibid., 41, p. 1, 1890.

[4] Wied. Ann 45, p. 508, 1892; ibid., 53, p. 845, 1894; et al.

[5] Wien Ber. 102 IIa, p. 913, 1893.

[6] Wien. Ber. 105 IIa, p. 139, 1896.

[7] Zeitschrift für Elektrochemie, 4, p. 29, 1897.

[8] Wied. Ann. 61, p. 1897.

[9] Proc. R. S. L. 64, p. 253, 1899.

[10] Phil. Mag. Sept. 1882, p. 186.

To determine whether, in addition to this electrostatic effect, there exists a true effect of electrification upon the surface tension of a liquid, investigations must obviously be made by methods which make the calculation of the purely electrostatic effect feasible, so that it may be eliminated from the total effect. Several earlier investigations bearing upon the existence and magnitude of a true change in surface tension produced by electrification, although quite inconclusive so far as such an effect is concerned, were reviewed by one of us in a recent paper.[1] This paper also described a new and more elaborate investigation of the same problem, and showed that the effect in question, if it exists at all, is much smaller than previously supposed. As the theory of the method used had not been fully developed at the time of publication, no more definite conclusion could then be drawn. To complete this theory, and by means of it to give the observations their final discussion, is the object of the present communication.

For a detailed account of the experiments, reference must be made to the paper already mentioned. It is sufficient to state here that the tension of the liquid surfaces, uncharged and charged to known electrical densities, was investigated by the method of capillary ripples, in a form capable of giving the surface tension of unelectrified water with an accidental error not greater than 0.05%.[2] When the liquids were electrically charged, either positively or negatively, the velocity of the capillary waves was found to be less than when the surface was unelectrified. In the case of water this observed change would correspond to a diminution in surface tension of about 4% for a surface density of 1.75 electrostratic units. For mercury the apparent change was not so great, being slightly less than 1% for a surface density of 2.33. It is to be observed, in accordance with what has been said above, that the results cited do not necessarily prove the existence of a *real* change in surface tension; for the electrostatic forces acting upon a charged surface would cause a diminution in the velocity of capillary ripples even if the surface tension remained unaltered. In order to determine whether a real change in surface tension results from electrification, we must take account of the electrostatic forces in deriving the relation between surface tension and velocity.

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[1] Barnett, PHYSICAL REVIEW, 6, p. 257, 1898.

[2] Barnett l. c., p. 263.

To determine the velocity of capillary ripples we have the general equation[1]

$$\frac{\delta p}{\rho} = -gh - \frac{d\varphi}{dt} \tag{1}$$

where ρ is the density of the liquid, h the displacement of the point considered above its equilibrium position, δp the resulting increment of pressure at the surface, φ the velocity potential, and g the acceleration of gravity.

If δp is due to capillarity only, we have

$$\delta p = -T\frac{\partial^2 h}{\partial x^2}$$

T being the surface tension; and equation (1) becomes

$$\frac{T}{\rho}\frac{\partial^2 h}{\partial x^2} = gh + \frac{d\varphi}{dt}\,. \tag{2}$$

For the case of plane sine waves such as we have supposed, this gives for the velocity v

$$v^2 = \left(\frac{g}{k} + T\frac{k}{\rho}\right)\tanh kl$$

l being the depth of the liquid, λ the wave-length, and $k = \frac{2\pi}{\lambda}$.[2]

In the experiments with which we are concerned the value of l was such that $\tanh kl$ differed from unity by less than one part in 2000, and we may put

$$v^2 = \frac{g}{k} + T\frac{k}{\rho}$$

with an error less than the experimental error.[3]

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[1] Rayleigh, Treatise on Sound, Vol. II, § 353, Second edition.

[2] Rayleigh, Treatise on Sound, Vol. II, § 353, Second edition.

[3] Barnett *l. c.*, p., 263.

If the wave frequency is n we have $v = n\lambda$, and the final expression for the surface tension becomes

$$T = \frac{\rho\lambda^2}{2\pi}\left(\lambda n^2 - \frac{g}{2\pi}\right). \tag{3}$$

When the surface of the liquid is charged, however, a pressure $-\frac{2\pi\sigma^2}{K}$ is developed in addition to that due to capillarity, σ being the electric surface density and K the dielectric constant of the medium above the waves. In order to obtain the whole increment of pressure, δp, resulting from the displacement h, it is therefore necessary to add to $-T\frac{\partial^2 h}{\partial x^2}$ a term $-\frac{2\pi}{K}(\sigma^2 - \sigma_0^2)$ where σ_0 is the surface density of the charge on the undistorted surface. In the place of equation (2) we have therefore, in this case,

$$\frac{T}{\rho}\frac{\partial^2 h}{\partial x^2} + \frac{2\pi}{K\rho}(\sigma^2 - \sigma_0^2) = gh + \frac{d\varphi}{dt}. \tag{4}$$

The surface density σ at any point of the wave surface is a function of h whose form remains to be determined.

In the experiments already mentioned the surface of the liquid was made one coating of an air condenser, the other coating being formed of tinfoil supported on glass at a height of about 35 mm. above the surface. This arrangement was adopted in order to obtain a more intense charge upon the liquid surface, and to make the electrical conditions definite, so that all the quantities involved should be capable of exact determination. To find the distribution of the charge over the wave surface under the conditions of the experiment, we may proceed as follows:

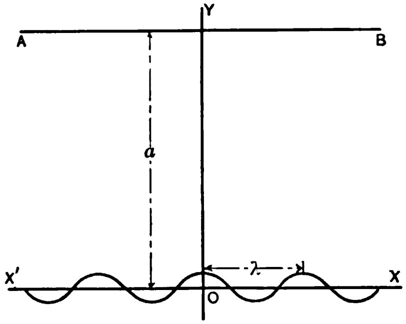

Fig. 1.

Let the axis of X be taken in the original level surface of the liquid, while the axis of Y is normal to the surface. Consider a train of plane waves of amplitude b travelling in the direction of X. As the waves proceed a rearrangement of the charge occurs; but this takes place so rapidly that the electrical conditions are always the same as though the ripples were stationary. The surface being at a potential V_0, while the plate AB is grounded, we have to determine the value of σ for all points along XX'.

For the region between the plates AB and the wave surface XX', the electrical potential V is a function of x and y subject to the condition

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} = 0. \tag{5}$$

It is clear that V, as a function of x, is periodic in the distance λ. With the origin as shown in the figure we may therefore write

$$V = A_0 + A_1 \cos kx + A_2 \cos 2kx + \cdots. \tag{6}$$

Since V must have the same value for $-x$ as for $+x$, none but cosine terms will appear in the expansion. The coefficients A_0, A_1, etc., are functions of y, but are independent of x.

Upon substitution in (5) we obtain

$$\frac{\partial^2 A_0}{\partial y^2} + \left(\frac{\partial^2 A_1}{\partial y^2} - k^2 A_1\right) \cos kx + \left(\frac{\partial^2 A_2}{\partial y^2} - 4k^2 A_2\right) \cos 2kx + \cdots = 0.$$

Since this equation holds for all values of x the coefficients must vanish separately. Therefore,

$$\frac{\partial^2 A_0}{\partial y^2} = 0, \quad \frac{\partial^2 A_1}{\partial y^2} - k^2 A_1 = 0, \text{ etc.}$$

The solution of these equations leads to the following values for the coefficients in (6):

$$\begin{aligned} A_0 &= M_0 y + N_0 \\ A_1 &= M_1 \epsilon^{ky} + N_1 \epsilon^{-ky} \\ A_2 &= M_2 \epsilon^{2ky} + N_2 \epsilon^{-2ky} \\ &\text{etc.} \end{aligned}$$

The constants of integration are to be so chosen as to satisfy the boundary conditions, viz.: (1) For $y = a$, $V = 0$ for all values of x: (2) for $y = b \cos kx$, $V = V_0$.

The first condition requires that A_0, A_1, etc., should separately vanish for $y = a$. Therefore

$$M_0 = -\frac{1}{a} N_0$$
$$M_1 = -N_1 \varepsilon^{-2ka}$$
$$M_2 = -N_2 \varepsilon^{-4ka}$$
etc.

For the coefficient A_1 we therefore have

$$A_1 = N_1 \left(\varepsilon^{-ky} - \varepsilon^{-2ka+ky} \right).$$

Since the expression for V is to be used only in order to find the force at the surface of the liquid, and from this the value of σ, we are chiefly concerned with its value for points immediately above the surface. For such points y is small when compared either with a or with λ. The exponent of the second term of A_1 for points in the immediate neighborhood of the surface is therefore practically equal to $-2ka$. In the experiments upon water the value of a (distance between the liquid surface and the plate above) was about 4 cm., while λ was always less than 0.5 cm. In the experiments upon mercury, the value of a was about 3.4 cm., and that of λ less than 0.37 cm. In all cases the quantity $2ka$ is therefore greater than 100, and the second term is the expression for A_1 is entirely negligible. This is also true of the corresponding terms in A_2, A_3, etc. The expression for V therefore takes the form

$$V = -N_0 \frac{y}{a} + N_0 + N_1 \varepsilon^{-2ky} \cos kx + N_2 \varepsilon^{-2ky} \cos 2kx + \cdots. \quad (7)$$

For points on the wave surface, where $y = b \cos kx$, the potential is V_0. Therefore for points on the surface

$$V_0 = N_0 - N_0 \frac{b}{a} \cos kx + N_1 \varepsilon^{-ky} \cos kx + N_2 \varepsilon^{-2ky} \cos 2kx + \cdots.$$

Remembering that the amplitude, b, of the ripples is extremely small, and that the y in the above expression can in no case exceed b, it is clear that the exponentials are practically equal to unity; and we have as a close approximation,

$$0 = N_0 - V_0 + \left(N_1 - \frac{b}{a} N_0 \right) \cos kx + N_2 \cos 2kx + \cdots.$$

Since this relation holds for all values of x, the coefficients N_2 N_3, etc., vanish, and

$$N_0 = V_0, \quad N_1 = \frac{b}{a} V_0.$$

The final approximate expression for V is therefore

$$V = V_0 \left(1 - \frac{y}{a} + \frac{b}{a} \varepsilon^{-ky} \cos kx\right) \tag{8}$$

and that for Y, the vertical component of the force at the surface, is

$$Y = - \frac{\partial V}{\partial y} = \frac{V_0}{a}(1 + kb \cos kx).$$

For waves of the extremely small amplitude used the horizontal component of the force is neglible in comparison with Y. Therefore,

$$\sigma = \frac{KV_0}{4\pi a}(1 + kb \cos kx).$$

Remembering that $\frac{KV_0}{4\pi a} = \sigma_0$, that $kb \cos kx$ is small, and that $b \cos kx = y = h$, we have, finally,

$$\sigma^2 - \sigma_0^2 = 2kh\sigma_0^2 = \frac{4\pi\sigma_0^2}{\lambda} h$$

and equation (4) becomes

$$\frac{T}{\rho} \frac{\partial^2 h}{\partial x^2} = \left(g - \frac{8\pi^2\sigma_0^2}{K\lambda\rho}\right)h + \frac{d\varphi}{dt}. \tag{9}$$

Upon comparing this with equation (2) it appears that the effect of electrifying the surface is to alter the velocity of the waves in the same sense and to the same extent as would a diminution of the acceleration of gravity by $\frac{8\pi^2\sigma_0^2}{K\lambda\rho}$.

The corrected expression for the surface tension [see (3)] is, therefore,

$$T = \frac{\rho\lambda^2}{2\pi}\left(\lambda n^2 - \frac{g}{2\pi}\right) + \frac{2\lambda\sigma_0^2}{K}. \tag{10}$$

The *apparent* surface tension *i. e.*, the value of T obtained from (3) [or from (10) without taking into account the final term, which gives the correction for electrostatic influences] is thus always less than the true surface tension by the amount $\frac{2\lambda\sigma_0^2}{K}$.

TABLE I.

Water.

V	λ	σ_0	$\frac{2\lambda\sigma_0^2}{74.3}$	T Apparent.	T Corrected.
0	0.496	0.00	0.00%	100.00%	100.00%
9000	.496	0.58	0.45	99.61	.06
12000	.495	0.77	0.79	99.29	.08
15000	.495	0.97	1.25	98.98	.23
18000	.494	1.17	1.82	98.37	.19
21000	.493	1.36	2.46	97.69	.15
24000	.492	1.56	3.22	96.94	.16
27000	.491	1.74	3.00	96.13	.13

TABLE II.

Mercury.

V	λ	σ_0	$\frac{2\lambda\sigma_0^2}{403.4}$	T Apparent.	T Corrected.
0	0.369	0.00	0.00%	100.00%	100.00%
9000	"	0.70	.09	99.92	.01
12000	"	0.93	.16	.88	.04
15000	"	1.17	.25	.83	.08
18000	"	1.39	.35	.75	.10
21000	"	1.63	.49	.61	.10
24000	"	1.87	.64	.44	.08
27000	"	2.09	.80	.25	.05
30000	"	2.33	.99	.08	.07

The experimental values of this apparent tension of water and mercury have been given for a number of different values of σ_0 in Tables IV. and V. of the paper referred to above, expressed in percentage of the values for $\sigma_0 = 0$.[1] By the use of equation (10) we are now able to apply the correction due to electrostatic influences. The results are given in Tables I. and II., expressed as before in percentage of the maximum values of T. These maximum values were 74.3 $\frac{\text{dynes}}{\text{cms}}$ for water and 403.4 $\frac{\text{dynes}}{\text{cms}}$ for mercury, respectively, which were obtained by making the following substitutions in (10):

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[1] Barnett, l. c., pp. 281–282.

$$K = 1, \quad g = 981, \quad n = 64.4, \quad \sigma_0 = 0$$

also, for water

$$\lambda = 0.496, \quad \rho = 0.998$$

and for mercury

$$\lambda = 0.369, \quad \rho = 13.54.$$

The final column of Table I. gives for the corrected surface tension of an electrified water surface values ranging from 0.06% to 0.23% higher than that of the uncharged surface, the average value being about $\frac{1}{7}$% higher; while the corresponding column of Table II. gives for the tension of an electrified mercury surface values from 0.01% to 0.10% higher than that for no electrification—the mean being about $\frac{1}{15}$% higher. The fact, however, that the very small variation follows no law in either case, being often less for high than for low electric surface densities, would indicate that it possesses no significance, and is due wholly to experimental errors. To make this evident, we shall examine the accidental error in measuring the "apparent" surface tension of water; what is said of the experiments upon water being true, with little alteration, for those upon mercury. As was shown in the paper already often quoted, a given percentage error in measuring λ produces in the computed value of T, under the conditions of the experiments, a percentage error about three times as great. More accurately, $\frac{\Delta T}{T} = 2.9 \frac{\Delta \lambda}{\lambda}$. An examination of figure 6 (*l. c.*) shows that in measuring λ the average departure from the mean (given by the curves) in each of the two measurements necessary was about $\frac{1}{20}$%. The average accidental error in T is therefore about $\frac{1}{4}$%; so that the small differences in the corrected value of the surface tension are all within experimental errors, and we may safely conclude that no effect of electrification upon the surface tension has been observed.

The complete investigation, therefore, while not excluding the possibility of discovering an effect of electrification upon surface tension by the use of improved methods and still more intense charges, itself gives no support to the view that such an effect exists.

AN OBJECTIVE REPRESENTATION OF THE HYSTERESIS OF IRON AND STEEL[1]

KNUT ÅNGSTRÖM.

THE very happy thought of using cathode rays for the study of alternating currents and of alternating magnetic fields appears to have had its origin with Mr. Braun.[2] The method employed is obvious from Fig. 1, which shows a tube of the kind furnished by Geissler in Bonn, according to Braun's specifications. Cathode rays from the terminal *K* pass through a small hole in the diaphragm *D*, and produce a brilliant spot of light upon that portion of the screen *S* upon which they fall. This screen, which is covered with phosphorescent material, fills the end of the tube. The approach of a magnetic pole at *D* deflects the spot of light. If a small magnetizing coil, through which alternating currents flow, be fastened beneath the diaphragm, the spot of light is thrown into a vibratory motion which is capable of analysis by means of the revolving mirror. If two coils, placed perpendicular to each other, with their axes at right angles to the axis of the tube are fastened in the neighborhood of the diaphragm, and each of these coils be supplied with alternating current, the spot of light under the influence of the two forces is given a combined motion analagous to Lissajous's curves.

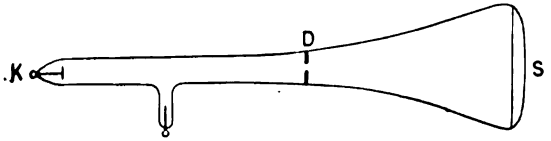

Fig. 1.

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[1] Translated from the transactions of the Royal Academy of Sciences, Stockholm, 1899, No. 4, by E. L. N.

[2] Braun. Wiedemann's Annalen, 60, p. 552, 1897; Elektrotechnische Zeitschrift 19, p. 204, 1898. The latter article is known to me only through an abstract in Wiedemann's *Beiblätter*, 22, p. 884, 1898.

Braun, in addition to his other experiments, has investigated the velocity of the transmission of magnetization in iron. Since these experiments bear a certain relation to some of those which follow, I take the liberty of describing them in a translation of his own words.[1]

"An iron rod, 2 m. long and 9 mm. in diameter, lies in a horizontal position, perpendicular to the axis of the tube, its end as near as possible to the diaphragm. Along this rod, a small magnetizing coil may be moved. A second coil precisely similar to this, is arranged in a vertical plane, likewise perpendicular to the tube, so that under the simultaneous action of an alternating current in the two spools, the spot of light describes a curve, the form of which is determined by the pole lying nearest to the diaphragm. Both coils are traversed by the same alternating current.

"If the coil is moved along the rod, the shape and position of the ellipse of vibration changes and when the middle of the spool is about 42 cm. from the end of the rod a difference of phase of $\frac{\pi}{2}$ shows itself which is independent of the strength of the current. From the number of oscillations (50) of the alternating current it follows that the velocity of 'propagation' of magnetic excitation is eighty-six $\left(\frac{\text{meters}}{\text{seconds}}\right)$, a value which accords very well with that found by Oberbeck under similar conditions (88.7 meters for a rod 8.7 mm. in thickness, with a vibration frequency of 133)."

As Braun himself remarks, we have to do here with complicated phenomena, and it is a question whether the value of the velocity of transmission thus determined is really that of the magnetic excitation.

While engaged in repeating some of Braun's experiments, I asked myself whether it would not be possible by means of his apparatus to represent objectively the hysteresis curves in iron and steel and to exhibit the same, and after several preliminary attempts I found a simple arrangement which led, in a most beautiful manner, to the result. If a piece of iron or steel is magnetized in a magnetic field, for example in a long magnetizing coil, and if we plot a curve with rectangular coördinates in which the strength of the field H is taken as abscissa and the induced magnetism I as ordinate, we obtain, as is well known (when H passes through values from H_1 to $-H_1$), a curve which we call the hysteresis curve, and which owes its existence to the fact that induced magnetism in iron does not possess the same value when increasing and when decreasing for equal values of H. This curve and the surface enclosed by the same, which represents a certain amount of energy consumed in the cycle of magnetization, may be determined in a variety of ways and when changes in the magnetizing force take place slowly the process is one which offers no great difficulties. The relations are, however, much more complicated when the intensity of the field changes rapidly.

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[1] Wiedemann's Annalen, 60, p. 557.

In the latter case losses of energy occur, as is well known, not only in consequence of the actual phenomena of hysteresis, but also in consequence of the action of Foucault currents. These which are generated in part by the inducing current in the mass of the iron, and in part by the induced magnetism itself, cause a retardation of the magnetic action upon the deeper layers of the substance (electro-magnetic screening). Attempts to separate these various factors from one another and to study them in detail have been made in recent times, among others by Ch. Maurain,[1] and by Wien.[2]

From their work one can form a clear conception of the difficulties with which such investigations are surrounded, and of the advantage of finding a simple method which would allow this relationship to be easily determined, which would make it possible to gain a ready acquaintanceship with the subject, and which, in other ways, would furnish a certain check upon the results. This has been attempted by Ewing,[3] who has constructed an apparatus that records the curves of magnetization automatically. In this apparatus a wire, which is placed between the poles of a permanent magnet, is traversed by a magnetizing current. Another wire carries a constant current and the latter is subjected to the action of the induced magnetism in the iron. The forces that act upon these wires turn a little mirror about two axes situated at right angles one to another. When the iron passes through a complete cycle of magnetization, a beam of light reflected from the mirror records upon a photographic plate the curve of magnetization. These and similar methods can, in the nature of the case, only be used where the changes in the magnetizing force take place slowly, since, as is the case with all mechanical devices, the inertia of the many parts introduces disturbances whenever the period of vibration is rapid.

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[1] Maurain; Annales de Chimie et de Physique; Series 6, 14, p. 208 (1898).

[2] Wien; Wiedemann's Annalen; 66, p. 859 (1898).

[3] Ewing; The Electrician; May, 1893.

First arrangement of my apparatus.—The first arrangement which I used will be easily seen from Fig. 2. The four coils, S, S_1, M, M_1, are fastened around the diaphragm in the manner shown. The dimensions of the coils are as follows:

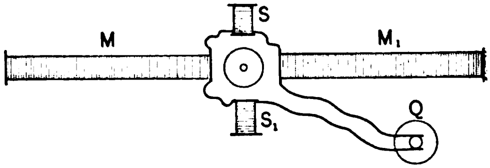


Fig. 2.

S, length 2 cm., external diameter 1.6 cm., internal diameter 0.5 cm., diameter of wire 0.3 mm.

M, length 20.5 cm., number of turns, 30 per second.

The coils, S and S_1 were wound upon wood, M and M_1 upon glass tubes. The current from a source Q passes through these four coils in series. It flows through S and S_1 (the indicator coils of Braun), in such a manner that the magnetic action of the same at the opening of the diaphragm is additive; through M and M_1 in such directions that their action at the point is annulled. The horizontal deflection produced by means of the action of the current in S and S_1 is proportional to the strength of the current and also to the magnetizing force. If a rod of iron be introduced into one of the magnetizing coils, the spot of light will be deflected in a vertical direction with a force which is proportional to the induced magnetism, so that under these circumstances the spot of light must pass through a true hysteresis curve.

Second arrangement.—The two indicator coils S and S_1 retain their places, but they are slightly displaced backwards and forwards respectively, with reference to the plane of the diaphragm. The magnetizing coils M and M_1 are placed parallel to each other and at equal distances from the diaphragm as shown in Fig. 3. The action of the current and of the magnetic field is the same as in the first arrangement. When currents of rather high intensity are used, it is easily possible to follow the movement of the spot of light directly with the eye. If, on the contrary, the cycle of magnetization is gone through with slowly, and especially if it is desired to measure the curve of hysteresis with precision, a photographic record is very advantageous. I set up the photographic camera in such a way that the images of the curves should be taken in half size. An exposure of from ten to twenty seconds suffices for the production of a good negative, which when developed and fixed, can be enlarged to any desired size by projection. The figures in the accompanying plate are reproduced directly from such negatives. The curves shown in the plate are from experiments upon hard steel wires 10 cm. in length and 0.175 cm. in diameter. I. was obtained from a wire containing 0.1 per cent.; II. from a wire containing 0.4 per cent. and III. from a wire containing 0.9 per cent. of carbon. I.–II., I.–III. and II.–III. are "difference curves" between these (I., II. and III.) taken two and two.

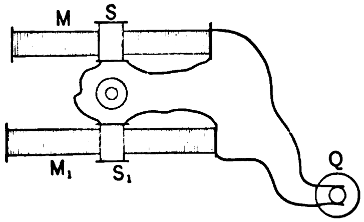


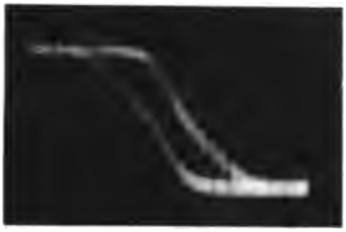
Fig. 3.

For very slow cycles, of magnetization (to produce static curves of hysteresis) I used as a source of current a storage battery, and varied the strength of the current by the introduction of liquid resistance, so as to change the magnetizing force between the desired limits. Alternating current was produced from a small hand-driven Siemen's alternating dynamo (from Ducretet in Paris) with which it was possible to vary the number of alternations between twenty and sixty per second. In this paper only a few examples of these experiments are given. I hope later to return to a more complete description of the results. Figs. 4–8 show drawings from some of the photographic records obtained with the second arrangement of the apparatus. The curves marked *A*, *B*, *C*, *D* refer to the following samples of iron:

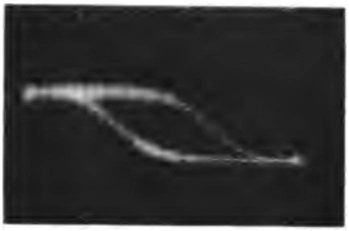
(*A*) A rod of iron; length 10 cm., diameter 0.3 cm., amount of carbon 0.2%; tempered. (From Bofors in Sweden.)

(*B*) A rod of iron; length 10 cm., diameter 0.3 cm., amount of carbon 0.8%; hardened. (From Bofors in Sweden.)

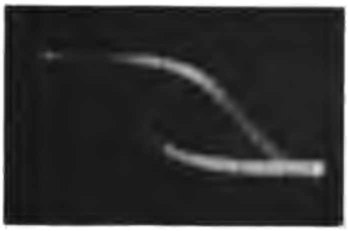
(*C*) A bundle of very soft tempered iron wire, length 10 cm., diameter of each wire 0.082 cm.



I



I–II



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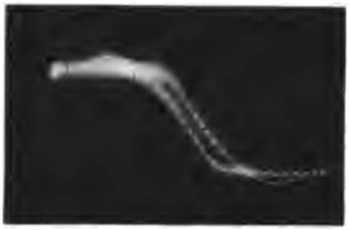
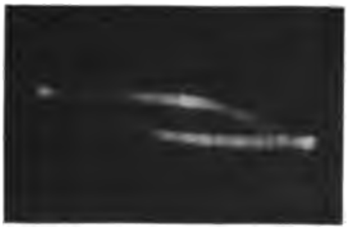
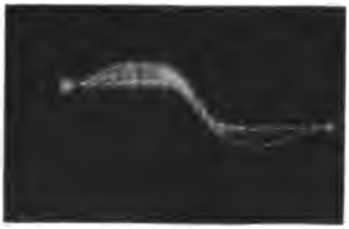


I–III



III



II–III

ÅNGSTRÖM: HYSTERESIS OF IRON AND STEEL.

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ASTOR, LENOX AND
TILDEN FOUNDATIONS.

(*D*) A precisely similar bundle of wires, surrounded by a tube of brass. External diameter of the tube 0.48 cm., internal diameter 0.30 cm.

Figure 4 shows the static curve of hysteresis. Figure 5 the corresponding curve for alternating current of about twenty reversals per second. Figure 6 shows the same curve for about sixty reversals per second. The maximum strength of the current in these experiments was maintained as nearly as possible constant, with a value of two amperes. In the static hysteresis curves obtained by this method, the influence of hardness is easily recognized. In the case of very soft iron there is naturally no difference between a massive rod and a bundle of wires. In massive rods of soft iron with increasing frequency of alternations, the loops change greatly but in the case of hard iron, the change is insignificant. That these

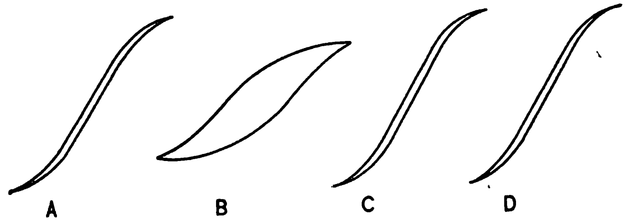

Fig. 4.

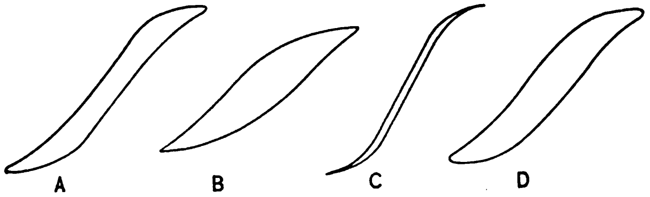

Fig. 5.

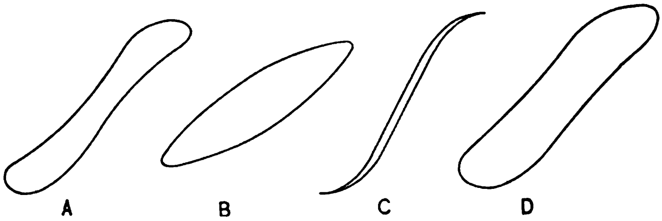

Fig. 6.

changes are almost exclusively due to the Foucault currents is evident from the experiments with the bundle of wires with and without the brass tube. The change in the loop with the number of alternations could not be detected in the case of the bundle, without the brass tube, in which case Foucault currents are almost completely excluded, but when the bundle was surrounded by the brass tube, the change in the loop was almost as great as in the case of a soft rod of massive iron.

In general, these results are in good agreement with the experiments of Maurain and of Wien in so far as they are capable of comparison with those investigations. Wien has used alternations up to a frequency of five hundred and twenty, and has studied the changes in the real hysteresis curves. Maurain finds that in the case of bundles of very fine wire, the loss of energy in the cycle of magnetization up to periods of fifty-five alternations per second is not appreciably dependent upon the frequency.[1] Unfortunately, I have not as yet had the means at my disposal to extend these investigations to a very high frequency.

A very interesting modification of the above mentioned experiment can be made by providing the magnetizing coils M and M_1 with iron cores. The curves obtained in this way are difference curves, which depend upon the varied permeability and hysteresis of the specimens, as well as upon the strength and difference of phase which the Foucault currents in the same possess. Figures 7 and 8 afford examples of such difference curves for two different frequencies, obtained by the used of the two rods, A and B, just described. In this case the difference curve for low frequencies is principally due to the difference in hysteresis. At higher frequencies, we have to do, in addition to the above, with unequal changes of phase of the Foucault currents. The curves in figure 8 were

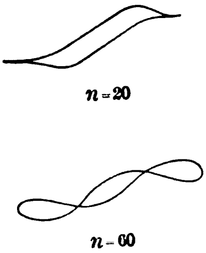

Fig. 7.

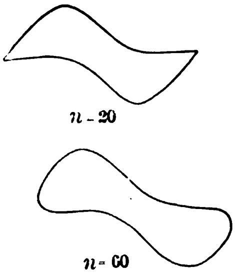

Fig. 8.

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[1] Maurain, l. c., p. 279.

obtained by means of the bundles of wires, *C* and *D* (with and without the brass rod). In this instance, the hysteresis remains unchanged, and the difference curve is to be ascribed exclusively to the action of the Foucault currents. This last named arrangement of the experiment affords a simple and practical method of testing various samples of iron in respect to their magnetic properties, and likewise to the amount of carbon which they contain.[1] If we place in the magnetizing coils two precisely similar rods of iron of the same character, they neutralize each other and the spot of light moves in a horizontal straight path just as if no iron were placed in these coils. If the rods, however, possess different properties, we obtain more or less well marked difference curves. If, therefore, we have samples of iron, the properties of which are precisely known, we can compare with them directly the property of unknown samples of iron of the same dimensions. The usefulness of this application of the method in electro-technical work, and particularly in the construction of dynamo machines, where hysteresis is of great importance, is obvious.

If, in the first arrangement of the apparatus the rods of iron are brought with their ends very near the opening of the diaphragm and if the magnetizing coils in the second arrangement are fastened directly above and below the rods, and we compare the results obtained in these ways, with one another, it is found, particularly if the iron rods are of great length, that there is a very marked difference between the two curves. The hysteresis curves trend more slowly upward and downward in the first case than in the second. I was for a time at a loss to explain these facts, but I believe that the phenomenon is due to changes in the magnetic distribution within the rods with increasing magnetization. The following experiments confirm this opinion. If we make use only of the two long magnetizing coils, and arrange these as shown in Figure 9, we find when the diaphragm is at the point *a*, symmetrically located with reference to the two spools and when a bundle of iron wires is thrust into one of the tubes, that vertical vibrations take place. If now the diaphragm is pushed along to the point, *b*, *c*, etc., the direction of vibration tends to turn itself in such a manner as to be always perpendicular to the lines of the magnetic field. At the same time, we notice, however, that the line of vibration as is shown in Figure 9, becomes curved at the ends. If we replace the bundle of wires with an iron rod, we no longer get lines of vibration, but complete loops. With increasing intensity of magnetization, the system of lines of force is displaced, and the whole action is like a shifting of the poles towards the ends of the magnetized rod. If this shifting is not the same for increasing and diminishing magnetization, or in other words, it is connected with a hysteresis effect, we obtain the above described loops. Through this displacement of the poles, we may likewise explain the above difference in the curves obtained with the first and second arrangements of the apparatus.

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[1] This comparison is based upon the assumption that the iron in other respects is of similar chemical and physical structure.

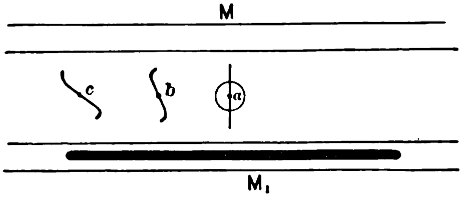

Fig. 9.

When short rods are used, and at a considerable distance from the diaphragm, the difference between the curves obtained is unimportant. The precise measurement of these curves would probably teach us much concerning the permeability and hysteresis of iron. Before entering upon this work in the case of certain varieties of Swedish iron, I purpose, however, to attempt important improvements in the cathode tube. I hope then to be able to work with currents of much higher frequency.

To Mr. Granqvist of the physical laboratory at Upsala, who assisted me in most of these experiments, I desire to express my heartiest thanks.

A DETERMINATION OF THE NATURE OF THE ELECTRIC AND MAGNETIC QUANTITIES AND OF THE DENSITY AND ELASTICITY OF THE ETHER, II.

Reginald Aubrey Fessenden.

Summary.

It was first shown that if we consider only those electric and magnetic phenomena which can be exactly expressed by equations containing only electric, magnetic and gravitational quantities, with those of length, mass and time, that there are an infinite number of theories possible.

If we suppose that the electric and magnetic quantities are such as we meet with in mechanics the number of possible theories will be much reduced.

We have no right to make this assumption, however, and the reduction cannot be regarded as rigorous, but rather as having a certain degree of probability or as serving as an indication, and hence can only be justified by showing that the results obtained by its means are in accordance with observed facts.

On this basis it was shown that one of two theories is the correct one, *i. e.*, that either κ is a density and μ a compliancy or μ is a density and κ a compliancy. It was then shown that if either coefficient is a compliancy, then, when the corresponding intensity is varied, the corresponding coefficient should vary in the opposite direction.

If neither theory were correct this change should not necessarily take place.

It was found that there is such a relation existing between one of the cöefficients and the corresponding intensity, *i. e.*, between μ and H, whilst there is no such relation observable between the other coefficient and intensity.

It was then shown that the rate of change of the coefficient μ with the intensity H was exactly that called for by the theory and a most rigorous experiment showed no deviation from the rate required not within the limits of experimental error.

It was shown that this relation held not only for iron and other strongly magnetic substances, but also for a wide range of substances whose reluctivities varied from 0.0003 to 1000, and was hence probably a universal relation.

Corroborative evidence was then given, showing that the reluctivity varied in different materials, in the same materials under temporary stress and in the same materials under permanent strain, in such a way as to accord with the theory. It was also shown that there was a relation between the coefficients of reluctance and of hysteresis which was also in accordance with the theory.

It was then shown that there was evidence of such a connection between capacity and density as agreed with the theory.

From this last relation the density and elasticity of the ether were determined, to a first approximation.

CONCLUSION.

Having determined the qualities of the electric and magnetic quantities, we may next proceed to more precisely determine their nature. For convenience we will here reproduce equations 12–15.

(12) $Q = M/T,$

(13) $P = L^2,$

(14) $\kappa = M/L,$

(15) $\mu = LT^2/M.$

The complete determination of these qualities is made as follows:

From equation 3, Maxwell's equation, we have

$$\kappa^{1/2}\mu^{1/2} = T/L = 1/\text{velocity}.$$

Let us take the X axis as in the direction of the lines of electric flux, the Y axis as in that of the lines of magnetic flux, and the Z axis as perpendicular to both.

The velocity of light is therefore to be measured along the Z axis. We know also that in the expression for volume, *i. e.*, L^3, the three lengths are to be taken in three different directions.

We have therefore

$$M/L_xL_yL_z \times \mu = T^2/L^2_z$$

whence $$\mu = L_xL_yT^2/ML_z.$$

Again, we have

$$B^2/8\pi\mu = \text{work per c. c.} = (P/\text{area})^2/8\pi\mu.$$

This work is done in the Y direction,

hence $$P = L_xL_y \text{ and } B = L_y/L_z.$$

From the equation

$$H\mu = B$$

we have $$H = ML_y/L_yL_xT^2 = M/L_xT^2.$$

To recapitulate,

$$B = L_y/L_z$$
$$H = ML_y/L_yL_xT^2,$$
$$\mu = L_xL_yT^2/ML_z.$$

From this we see that a current flowing in a circuit (this being in the X axis) produces a twisting force around the axis of the wire, H being the intensity of the force, or the force per sq. cm. This force produces a shear, B being the amount of the shear, and the shear at any point depending upon the distance from the wire. The amount of the shear for a given force per sq. cm. depends upon the resistance of the medium to shearing stress, and $1/\mu$ is the rigidity modulus of the medium. Hence $B = H\mu$.

In the presence of iron the virtual rigidity is greatly reduced. The stress H produces a shear B_e in the ether numerically equal, if we take the permeability of the ether as unity, to H. The same stress also produces a virtual shear B_i, ($= 4\pi \times$ intensity of magnetization). The sum of these two shears makes up the total shear B, or $B = B_e + B_i$ (equals numerically $H + 4\pi I$).

The shear B_e increases directly as H. Since the greatest value of H which we can reach is not more than 100,000, which corresponds to a force of only about 4.10^8 dynes per cm. and the rigidity of the ether is, as we have found, approximately 6.10^{20}, we could hardly have expected it to be otherwise, as the greatest shear which we can produce is through but an infinitesimal angle.

The shear B_i (for it is virtually equivalent to a shear), increases as H increases, but reaches a limit, for iron, at about $B_i = 25{,}000$. It does not increase however, directly as H, but as we have seen the two are connected by the formula

$$B_i = H/(a + bH).$$

Under certain circumstances, for example with stressed nickel, as shown in Fig. 4, the constant a is reduced to zero and the intrinsic shear takes its full value with an indefinitely small value of the stress H, any additional increase in B being due entirely to increase of the extrinsic flux.

It will be noted that P/μ is a force in the Z direction.

Taking up next the electric quantities, from the formula

$$D^2/8\pi x = (Q/\text{area})^2/8\pi = \text{work per c.c.}$$

we get, since the work is done along the X axis,

$$Q = M/T$$

$$\text{and } D = M/L_yL_zT.$$

From the formula

$$QE = \text{work},$$

the work being done along the X axis,

$$E = L_x^2/T$$

$$F = L_x/T.$$

To recapitulate,

$$D = M/L_yL_zT$$

$$F = L_x/T$$

$$x = M/L_xL_yL_z$$

We may interpret this as meaning that a varying magnetic field produces a flow in the ether in a plane perpendicular to the direction of the magnetic lines, which flow persists, with varying velocity, so long as the amount of the shear is changing. The electric intensity F at any point is the velocity of the flow at that point. The capacity, x, is the density of the flowing medium and the displacement per square cm. D, is the current density.

If we have a condenser with a dielectric part paraffin, of density 0.9 and part vacuum, then since the electrostatic induction is continuous through the interface, *i. e.*, M/L_yL_zT is constant, the velocities must be different for each side of the interface.

The density of the ether is 0.6, *i. e.*, there is that much inertia per c.c. in the vacuum. In the space occupied by the paraffin the total amount of inertia is $0.9 + 0.6 = 1.5$.

Therefore, since the flow per sq. cm. is constant, and the densities vary in the ratio 0.6 : 1.5, the velocities, *i. e.*, the electric intensities, must vary in the inverse ratio. Hence the slope of electric potential changes at the interface in the ratio 1.5 : 0.6.

But the energy is given by the formula $mv^2/2$. Therefore in the ether the energy per c.c. is 5/3 of that in the paraffin.

The velocity in the ether corresponding to any voltage may be calculated, on the assumption that all the ether flows. We have

$$xF^2/8\pi = \text{energy per c.c.}$$

and x is 0.6. If F is one electrostatic unit and x is taken as 1 for the ether, we have,

$$v^2 \times 0.3 = 1$$

and $$v = 1.8 \text{ cm. per sec.} = F/\sqrt{4\pi}.$$

Since a volt is 1/300 electrostatic unit, the velocity corresponding to a volt is 0.006 cm. per sec., or about 20 times the velocity of the hydrogen atom when electrolyzed in aqueous solution.

We might naturally look for an effect upon the velocity of light in the direction of the lines of electrostatic force, since F is a velocity. Such an effect was discovered by Kerr, who has shown (Phil. Mag., April, 1894) that only the component vibrating in the plane of the lines of force is changed in the phenomenon which he discovered, the component perpendicular to them being unaffected, but the phenomenon is complicated here by the presence of matter, the effect is so large that it cannot be due directly to the flow of the free ether, and does not vary directly as F.

Rayleigh has examined the effect when light is passed through a solution carrying a current, without observing any increase however, as might be expected, since one volt per cm. corresponds to a velocity of only 0.006 cm. per sec. and the velocity of light is 3.10^{10} cms. per sec.

We can, however, without any extraordinary difficulty, obtain a voltivity of 150,000 in a vacuum tube, and this would mean a velocity of 800 cms. per sec., which, I learn from my colleague, Professor Wadsworth, should be observable with the refractometer. The experiment will be made.

It may be mentioned here that in doubly refractive substances the piezo-electric effect along different axes should vary with the refractive indices.

To consider next electromagnetic phenomena. We must first find the quality of the curl from the following two equations, representing the effect of a varying electrostatic flux or an electric current, and of a varying magnetic flux :

$$\mathrm{curl}_x\, Q/T = G = H \times L_y \text{ and } \mathrm{curl}_y\, P/T = E = F \times L_x$$

whence

$$\mathrm{curl}_x \text{ (curl round } X \text{ axis)} = L_y/L_x$$

$$\mathrm{curl}_y \text{ (curl round } Y \text{ axis)} = L_x/L_y.$$

It is first to be noticed that a moving flux and a current are not identical in effect. A rotating magnetized ring produces no voltage of the first order of effect in a coil surrounding it. Nor can a rotating glass ring with an electrostatic flux passing round the torus, magnetize to any degree an iron ring encircling it. The movement of the electric lines in a lateral direction is what gives the gilbertance and the lateral movement of the magnetic lines the voltage. Longitudinal motion of the flux has no effect, as there must be a change in the amount of the enclosed flux.

Just outside a wire carrying a current the lines of voltage are almost normal to the surface of the wire, as was pointed out by Heavyside, who discovered the true method of establishment of current in a wire. So long as the ends of the lines are stationary with reference to the wire, there is no current and no magnetic force. But when they move, the original velocity along the lines has compounded with it the lateral velocity of the lines, and the acceleration of the ether measures the value of H at any point. When we establish an electrostatic field between the plates of a condenser, the flow between the plates causes, in some manner, a tension normal to the surface of the plates and a lateral pressure. Whilst charging the plates we are increasing the lateral pressure. This creates a shear in a direction at right angles to both tension and pressure, just as, in thrusting a ball sidewise between two other balls, the side pressure thrusts the two balls apart and a force at right angles to the lateral pressure is thus produced.

To some extent the above description is founded upon assumptions. For example, it does not follow that because F is a velocity and because it is the velocity of the ether (since we can have a value of F in a vacuum), that all the ether in a cubic centimeter of space is moving when we have a voltage there. This however does not affect the *nature* of the quantity but only the configuration.

I have not suggested any detailed theory of the configuration, for the reasons given in the preface. There are a number of experiments to be made before we can go further with any degree of certainty. Any detailed work on configuration would imply a theory of the nature of matter, which is foreign to my subject. We may of course choose some configuration to work with. In doing this the first thing to be looked after is to have the flux of ether between the two charged plates of a condenser return again, as in a closed circuit. True there is no real reason for this. Continuous flow, without return, is inconceivable, but that is not a very serious objection.[1] To avoid having sinks, however, we may choose some suitable configuration. Possibly the best available is that given by Fitzgerald in his paper "On a Hydrodynamical Hypothesis as to Electromagnetic Actions."[2] According to this, the electrostatic line is a spiral vortex. This gives us our tension between the plates, and the lateral pressure. It can also give us the magnetic shear, and in the paper referred to it is shown that the equations for wave propagation and energy in such a medium as described are identical with those of light. We may also take the atom itself as a spiral vortex, and can get a secondary stress, depending upon angular velocity, to account for gravitation, which would be the gravitational stress. As we have found the rigidity of the ether to be

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[1] It is not many years since one of our greatest and most esteemed philosophers (Herbert Spencer, First Principles) came to the conclusion that the idea of an indivisible atom was inconceivable. As a matter of fact it *was* inconceivable then, but it is so no longer. One is reminded of Beaconsfield's witty note to his allegorical history of England (Isle of Fantasia, Chapter IV.) "First principles are the ingredients of positive truth. They are eternal and immutable, as may be seen by comparing the first principles of the eighteenth century with the first principles of the nineteenth."

[2] Royal Dublin Soc., December 12, 1898.

6.10^{20} this would permit of even the large values which Maxwell has pointed out are necessary with a stressed medium theory of gravitation.

Under these circumstances we might expect the permeability of a material to vary with a change in the force of gravity, and a charged condenser to weigh more than an uncharged one. I have not calculated whether the effect should be measurable or not.

In a work extending over many years I have naturally incurred indebtedness on many sides; I have more especially to express my gratitude to my present colleague, former assistant and sometime pupil, Professor S. M. Kintner, without whose invaluable constructive and experimental ability I should have been able to accomplish but a small portion of the needed experimental work. I have also to thank two other of my colleagues, Dr. Brashear and Professor Frost, for their kind help in many ways. To Helen M. Fessenden also I am indebted for much of the work done in reducing observations and in the preparation of tables.

Appendix A.

Investigation of the Relation Between μ and H.

It is evidently desirable that the correctness of this relation should be established with some degree of precision.

It was first put forth by Kennelly in the *Trans. Am. Soc. Inst. E. E.*, Oct. 27, 1891, and has been confirmed by Steinmetz, in a most elaborate and valuable paper on magnetism.[1]

It has also been verified by the author, in a research conducted expressly for that purpose, the method and results of which are given below.

Frölich's formula is $1/\mu = c + dH.$

Kennelly's formula is $\{1/(\mu-1)\} = a + bH = \nu_i.$

The difference between the two is that Kennelly considers, as mentioned above, the induction contributed by the iron as separate and distinct from that contributed by the ether, in the same manner as, in Gladstone's law, we consider the refraction contributed by the matter as distinct from that contributed by the ether. There are thus two magnetic circuits in parallel; one, the intrinsic circuit, carrying a flux numerically equal to $B - H$, the other the extrinsic circuit, carrying a flux numerically equal to H. As they have both the same difference of magnetic potential across them, the magnetic resistivity, or reluctivity, is, in the two cases, numerically $H(B - H)$ and H/H.

Kennelly's law means that this intrinsic reluctivity, ν_i is given by the formula $a + bH$, or in other words, that this intrinsic reluctivity varies as the space rate of drop of magnetic potential H varies.

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[1] Trans. Am. Inst. E. E., Sept. 27, 1892.

Frölich's formula fails for high magnetizations. Kennelly's does not. Kennelly's formula gives a constantly increasing intensity of flux, with increasing *H*. Frölich's formula gives the intensity as reaching a limit. This former appears from Ewing's experiments by the isthmus method to be the correct statement.

As will be seen from the papers of Kennelly and Steinmetz, the agreement of the formula with theory is fairly close in all cases examined. It had however been stated[1] that it was a purely empirical formula and was not the expression of any physical law. The writer, therefore, made a set of tests with a view to obtaining results of as high a degree of accuracy as might be possible.

In taking curves of magnetism, the following points are important:

1. Long wires cannot be used, as the leakage varies with the reluctivity, and the area of leakage surface is relatively large compared with the cross section. At low values of μ the results may be many per cent. out.

2. Rings wound from wire cannot be used, as the magnetic flux has to jump from one wire to another through an air gap, and thus an additional reluctance is introduced, and moreover the inductance is not uniform, and the true shape of the curve is greatly masked. Steinmitz has shown[2] that when the flux has to pass in this way the variation from the mean induction may be so great as to increase the mean hysteresis 70 per cent. in sheet iron, and no less than 900 per cent. in iron wire, above the hysteresis corresponding to the mean induction.

3. Rings punched out of sheet iron are the only ones permissible, as it is not possible to weld a solid iron ring and keep its constitution and fiber uniform. Moreover, magnetic creeping is worse in solid rings.

4. The outside diameter of the ring must not be more than ten per cent., preferably not more than five per cent., greater than the interior diameter. In a ring of interior diameter 6 inches, external diameter 10 inches, which the writer once saw used for the determination of a permeability curve, it is evident that when *H* was 3 on the outside circumference of the ring it was not less than 5 on the inside circumference. Consequently, when the outside portion of the ring was on the downward part of the reluctivity curve, the inside part was on the upward side, and the curve obtained from such a ring must be entirely misleading, the features of the curve being run together, and the true reluctivity being at one point much lower than it would appear to be from the test.

This fact, that the diameter of the ring may affect the results by five per cent. or more, has been pointed out previously by Kennelly.

5 Rheostats with sliding contacts cannot be used for this work, as they give very insidiously distorted curves. This is due to the fact that if we are at *P*, Fig. 6, and the resistance in the circuit is, say, 200 ohms, and the next resistance 400, then on moving the contact the resistance does not change from 200 to 400, but from 200 to, say, 800 ; and from 800 to 400, due to the fact that when moving from one section to another the contact is not very good, and though of course not necessarily broken with a properly designed rheostat, yet the contact is worse when moving from one section to another than when at rest.

The result is, curves of the shape shown in Fig. 6 in the dotted line, where *E*, after sinking down lower than it should, comes back to a lower point than it would otherwise have fallen to. The result is a flattening, or even a concaving of that part of the curve.

A similar thing occurs on the up curve, and on the plain induction curve. The distortion is small, but since the curve is very steep, it makes a large percentage error in calculating the ferric reluctivity.

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[1] Elec. World, June 9, 1894.

[2] On Hysteresis, Trans. Am. Inst. E. E., 1892, pp. 699 and 701.

6. All throws of the galvamometer must be of the same magnitude. This because the law of the galvanometer cannot be depended upon, the theoretical laws being only approximately followed and not closely enough where an accuracy of $\frac{1}{10}$ per cent. is required. A much more important reason is that the amount of creeping seems to be approximately proportional, according to some experiments of the writer's students, to the change of induction. It is therefore evident that, by changing B by a constant amount each time and taking approximately the same time for each throw, the results will probably be more reliable than when working more or less at haphazard.

7. If we change H to H' and then B to B' the state reached at B' will not be stable, even after creeping has stopped.

For if we wind on the iron ring a few turns of wire through which a very feeble current may be sent, which we call dH, then, on changing H from say 3 to 4, and then observing the value of B, if we next throw on dH so that its gilbertance is in the opposite direction to that of the main coil, we shall find that B is diminished by, say 50 lines. If, however, the direction of dH is the same as that of H, then B will be increased by, say 5 lines only. This would be bad enough, but the matter is still further complicated by the fact that if, after changing H from 3 to 4 and then throwing dH on in the same direction as F, giving an increase of from B to $B' + 5$, we then reverse dH, B' does not fall, as might be expected from $B' + 5$ to $B' - 50$, but only to $B' - 5$. What the ultimate value of B' is, then, when $H = 4$ and dH has been removed, depends upon which direction dH was first applied.

It is, therefore, impossible to get accurate magnetic curves with a dynamo current, as the minute fluctuations affect the result, and may not affect it twice in the same way.

This effect was investigated by one of the writer's students, Mr. Davis,[1] in 1896–7. The amount of the effect seems to depend upon the permeability. For this reason the step by step method was used in preference to that of reversals.

An additional reason for using a battery current is that small changes of H due to small pulsations in the current, as the brushes pass over the commutator segments, seem to change the rate of creeping, and also the amount.

The existence of the above phenomena is an additional reason against using a rheostat with sliding contact.

8 The time constant of the circuit must be small as compared with that of the galvanometer, as otherwise the system will begin to move before all the discharge has passed.

Sometimes the time constant is larger than would be expected without calculation.[2]

In the test about to be described the above effects were taken into account, as well as others, better known, due to thermo-electric currents, leakage, heating of resistance, and lack of uniform temperature.

The rings were of the best kind of transformer iron, cut to size on a tinner's machine, not punched, and carefully annealed. They were kindly furnished by the Westinghouse Co. In other samples, where the method of annealing was such that gas or carbon might be absorbed in the annealing, I have found that good results were obtained by enclosing the rings, embedded in sulphur-free iron peroxide, in a cast-iron box, with sides one inch thick. Some of the peroxide will be reduced, and it can be seen whether the gas has got to the plates or not by the difference in color of the oxide.

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[1] Thesis, "On the Closed Magnetic Circuit in Telephones."

[2] In the case of the field rings for the Niagara generators, the time constant was measured in minutes, and Mr. Scott and the writer had, therefore, to devise a method in which the time rate of change of induction was kept constant, and the time for the complete discharge was the thing noted.

Each individual ring was examined to see if the coating of oxide was abnormally thick or held much magnetic oxide. If so, it was rejected. So far, the writer has been unable to find any way of getting at the true cross-section of the iron without destroying the rings in cracking the scale off, but for the present purpose that was unnecessary, as relative values only were required and a mistake as to the cross-section would affect all constants in the same ratio. The accepted rings were measured across three diameters, inside and outside, on the bed of a comparator.

This may seem an unnecessary refinement, but it was the shortest way, and it was thought best to know the mean circumference with some degree of accuracy.

The rings were then shellaced, dried, built up into one thick ring, 10 inches inside diameter, 12 inches outside diameter, and 1 inch thick. This was then carefully insulated with varnished paper, and both primary and secondary wound symmetrically, *i. e.*, the wires spired evenly round the whole of the circumference so as to prevent uneven distribution of magnetic leakage. Then the whole was shellaced and dried.

The galvanometer was astatic, of a well known type, with four bell-shaped magnets in the system, being rendered more or less sensitive by screwing up a ring on one of the magnets.[1]

It was enclosed in a cylinder of soft cast iron, with top, sides and bottom of a thickness of 6 inches. The mirror was true, and the cover glass of the galvanometer was a parallel plane, ground by the writer at Mr. Brashear's shop.

The scale had been tested by the Société Genevoise comparator mentioned above. The telescope was of the usual type, and special arrangements were made to secure good lighting of it.

The suspension was a quartz fiber, 6 inches long. The period of the system was 20 seconds. reached by laying a piece of a magnetized file inside the iron case between the bottom of the case and the bottom of the galvanometer and moving it about until the desired sensibility and position were obtained.

The variable resistance was made after the fashion of a Varley rheostat, two wires wound in grooves on two pairs of wooden cylinders 1 foot in diameter. But along one cylinder of each pair was laid a metallic strip with holes drilled in it. In these holes small copper plugs could be screwed, in such fashion that where there were no copper plugs the wire passed over the strip, but where a plug was inserted the wire rested on it and so made contact with the strip. In turning the cylinder the wire was wound up until it reached the groove where there was a copper plug, when the contact was made and the resistance of the circuit decreased by the amount desired. In hysteresis tests, one pair is used for the up curve and one for the down.

A preliminary run was made between the values of H desired, with the plugs arranged by guess. From this the position in which the plugs must be placed to give equal deflections on the galvanometer were calculated, and the plugs so placed. The wires were German silver, of ample carrying capacity, and the heating very light owing to this and to the large surface exposed to radiation. The ends of the pins were grooved so that the contact should be good and sharp.

The iron was demagnetized by careful reversal, in diminishing cycles, combined with tapping, the ring being hung so that the plane of the ring was perpendicular to the direction of the earth's total magnetic field. The ring at first had an alternating current generated in it, with a view to shaking out the magnetism,[2] but this was afterward abandoned, as it was thought that it might introduce errors by possibly leaving some circular magnetization. The current was measured by a Weston milliammeter which could be read to $\frac{1}{15}$ per cent. of the maximum magnetizing current used, *i. e.*, about 1.45 ampères. The proportionality of this scale had been corrected for a particular position and orientation in the laboratory, by an accurately calibrated slide wire bridge, almost free from thermo-electric effects. Its absolute constant was determined by a Kelvin centi-ampère balance, whose constant had been frequently determined, but not much care was taken with this, as only relative values were desired and any great accuracy as regards this point would indeed have been absurd, in view of the fact that the true cross-section of the iron could not be accurately known within several per cent., so that absolute values were unobtainable.

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[1] Willyoung, Elect. Eng., N. Y., Nov. 16, 1892.

[2] Ewing, Mag. Ind. in Iron, p. 318.

The ring was suspended in slings in a wooden frame, to prevent vibration and allow free radiation, and placed so as to be free from all outside magnetic influences. The temperature of the galvanometer, ring and ammeter were known. The measurements were made in the spring of the year, and during the hour which each set took, approximately, the temperature of the basement room did not change, as a rule, more than two degrees, whilst the galvanometer temperature change was so slight (in its double covering), that it never reached $\frac{1}{10}$ degree, which was as small a change as the thermometers used could show. The wire used on the iron ring, consisting of flat strands of fine wire for both primary and secondary was of ample cross-section, for the double purpose of keeping the heating negligible and of having that part of the resistance outside the galvanometer box as small as possible, so that changes in the room temperature would have but a small effect on the total resistance of the secondary circuit.

In making the test, approximately the same time elapsed between each throw. Each throw was about 15 inches, and as the inches were divided into tenths and each tenth could be read to the decimal part, the accuracy of the reading must have been approximately $\frac{1}{10}$ per cent. The constant of the galvanometer was determined, roughly, as accuracy was not necessary here, by a mutual induction coil, wound on glass. By means of this coil and the Weston milliameter mentioned above, the relation between quantity and throw was very accurately determined, especially for that part of the scale which the readings obtained in the experiment proper fell upon.

The third and best curve obtained is given in Fig. 8. The dots representing the observations are covered by the line, the greatest deviation from the line being almost exactly $\frac{1}{8}$ per cent. of the maximum ordinate, when all corrections were made. Below $H = 2\frac{1}{2}$ the line begins to deviate from the formula, and below $H = \frac{1}{2}$ the agreement is not even approximate.

As a further example of the accuracy obtainable by this method it may be said that a set of six hysteresis curves was taken, in the following year, by Mr. Kennelly and the writer in conjunction, with the same apparatus (with the exception of the resistance cylinders, for which were substituted separate coils hanging in mercury cups and suspended in mineral oil, being short circuited or cut in circuit, as it was desired to vary the current) and, with much more unfavorable temperature conditions, the worst hysteresis curve obtained closed up to within $\frac{1}{4}$ of one per cent. of the difference of B between $H = +$ max. and $H = -$ max. the best curve being out $\frac{1}{10}$ per cent., and the error of the other curves lying between these values. Without the above mentioned precautions the writer has never been able to *depend* upon hysteresis curves closing up within two per cent.

The curvature at the commencement is always found. It becomes much more sharp when the difference between the inside and outside diameters of the ring is taken into account.

If not, the error may amount to several per cent., and, in fact, for accurate results this must always be allowed for.

In the curve given, Fig. 7, a curve was drawn first from the corrected observations.

Then from the values of a and b obtained from this first curve, an allowance was made for the error caused by the mean permeability not being the permeability of the mean length. From this the correct curve was drawn, it not being necessary to make a second approximation, as this would alter the values by less than the experimental probable error.

The elbow may be removed, as shown in the following curves, Fig. 5, taken from the observations of Gerosa and Finzi,[1] which show that when the iron is shaken up the elbow disappears, but that a and b are not greatly changed.

It is thought that the above experiment, made with a view to testing the matter as accurately as might be, proves rather definitely that we have here no rough empirical formula, but the expression of a physical fact; *i. e.*, that the intrinsic reluctivity is a linear function of the drop of magnetic potential per linear cm. This may be considered the touchstone of the electrical theories, and as proving without doubt that κ is a density and μ the reciprocal of an elasticity.

The following formulæ are given, as being of use in electromagnetic design :

$$(1, 2, 3)\quad \nu_i = a\Sigma_0^\infty bB = a + bH = a/(1 - bB).$$

$$(4)\quad B = H/(a + bH). \qquad (5)\quad H = aB/(1 - bB).$$

$$(6)\quad .4\pi nI = P\left[\frac{aL'c}{R - bPc} + \frac{a'L'c'}{R' - b'Pc'} + \cdots\right].$$

$$(7)\quad \frac{dB}{dH} = a/\nu_i^2 = aB^2/H^2. \qquad (8)\quad \frac{b\eta}{a} = .007 \text{ approx.}$$

Where B has been, for practical purposes, taken as equal to B_i, in nos. 4, 5, 6 and 7. I represents current, n = number of turns, P = total magnetic lines, B = lines per sq. cm., H = difference of magnetic potential per cm., $\nu_i = 1/(\mu - 1)$, L = length of part of magnetic circuit considered, R = cross-section of same, c = leakage coefficient of same, η = hysteresis loss per sq. cm. per cycle.

APPENDIX B.

RÉSUMÉ OF PREVIOUS WORK.

It was pointed out, in another portion of this paper, that given a knowledge of the nature of the electric and magnetic quantities, there is no difficulty in inventing suitable theories in regard to configuration, the number of such theories being chiefly a matter of permutations and combinations, but that each theory of the configuration virtually implies a theory of the constitution of matter.

In the present appendix it is not my intention to give an account of all the work done during the search for a solution of the problem with which this paper is concerned, for a record of this work, which has occupied a considerable portion of my leisure during a period of ten years, would be very voluminous. I shall deal only with that part of it which tends in some way to throw light upon the nature of the atom, and with that but very briefly. The results will be given in logical order and not in the order in which they were obtained.

PROPERTIES OF THE ATOM.

Various estimates have been made of the size of the atoms. There is, however, this question to be first answered : "Have the atoms definite sizes?"

If we take 45 c. c. of potassium and 18 c. c. of solid chlorine they can unite chemically to form a quantity of KCl occupying a space of but 37 c. c. One natural explanation of this fact that has been suggested, is that even in a solid the atoms do not lie closely adjacent to each other and hence there is abundant space for contraction even after the addition of other atoms. But this appears to be negatived by a number of facts. In the first place it is difficult to see how fusion can mean anything else than that the atoms have been driven so far apart by reason of the increase of kinetic energy that they can slide over one another and take each other's places. Hence in a solid it seems reasonable to suppose that they are so close together that this sliding over is impossible. Secondly, from Van der Waal's equation we may make an estimate of the volume of the atoms compared with that of the space occupied by them at the critical volume, and, hence, if we further deduce what this ratio must be for the solid state we see that the atoms must be nearly touching each other in this latter condition. Again, the fact that the electric conductivity and some other properties vary directly with the temperature as the temperature is lowered whilst these same properties are much altered by change of density, renders it hard to believe that there is any abrupt change in the volume of solids as we approach the absolute zero, which would apparently be necessitated by this explanation.

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[1] See Ewing, Mag. Ind. in Iron, p. 319.

A second theory is that of Shröder, who assumes that the atom may have two or more different volumes, the volume of the atom depending upon the nature of the chemical combination.

It has been shown by the writer[1] that neither of these suppositions is necessary. If we take 100 balls of pitch,[2] each having a diameter equal to that of a cube containing .45 c. c. and stack them in rectangular array, as shown in Fig. 1, *a*, they will occupy a space of 45 c. c. We may assume the rectangular order as that naturally taken by bodies attracting each other and at the same time repelled by their mutual collisions. If the repelling force be increased by further heating, they separate further and can now slide over one another. If the kinetic energy be lessened, or some additional attractive force brought into play, the atoms come closer together and the substance is no longer isotropic but tends more to be crystalline.

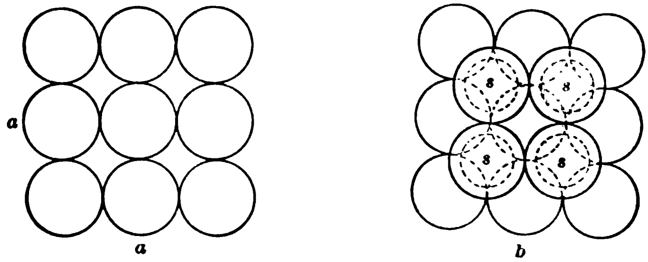

Fig. 1.

Take now 100 other balls, each having a diameter equal to the side of a cube whose volume is .18 c. c. Stack them in rectangular order and they will occupy a space of 18 c. c.

Next arrange alternate layers of the large and small balls, as shown in Fig. 1, *b*, where the attraction of the chlorine atoms is supposed to have pulled the potassium atoms over,

[1] Elect. World, Aug. 22, 1891. Science, March 3, 1893.

[2] Best made by pouring a slab of pitch, cutting up to *weight* and rolling between boards covered with pigment. Or, if a squirting machine is available, by cutting up squirted rod in proper lengths and rolling as before.

as shown. On measuring the volume now occupied by both sets of balls it will be found to be nearly 37 c. c.

This simple experiment may not be considered convincing. But when we find, on applying the simple formula thence derived,[1] that we obtain not only the correct molecular volumes of KCl, NaCl, KOH, NaOH, Cæs. Cl, Rub. Cl, and other salts, and when in addition we find that in most cases so far tested the experiment also gives the correct crystalline angles,[2] we are to a certain extent justified in holding that

A. The space occupied by the atoms of a metal to the exclusion of other atoms is of an approximately spherical shape at ordinary temperatures.

B. The atoms of a metal, the metal being in a solid state, are not widely separated, but the shortest distance between the surfaces of two adjacent atoms is of the order of one per cent. of the diameter of the atom itself, when at temperatures not near the fusion point.

Metalloids, as we know from their low specific heats, form molecular groups in the solid state. Hence we may have more than one melting point, first when molecule can slide over molecule, and second when atom can slide over atom. If the atom or molecule be very irregular in shape, a separation sufficient to permit of this sliding may so weaken the cohesive force that it is overbalanced by the kinetic repulsion and the substance gasifies without melting.

Since also we find that it is possible to predict the crystalline shape of but few of the metalloids by means of the method mentioned above, we are led to consider the following statements as probable. For definition of metalloids see Fig. 2.

C. Metalloids differ from metals in that their atoms have not an approximately spherical shape.

D. The melting point and expansion of the elements is a function of the shape of the atoms.

Cauchy, in 1835 made the first estimate of the size of the atom, and in 1870, Kelvin by other methods arrived at the conclusion that the mean distance between the centers of contiguous molecules was less than 10^{-8} cm., and more than 5.10^{-10} cm.

Recent determinations by different methods agree quite closely, and it would seem as if the mean of the results cannot be far from the truth.

Given by.[3]	**Value.**
Nernst, Theoretical Chemistry	2.58×10^{-8}
Nernst, " "	2.69×10^{-8}
Risteen, Molecules and Mol. Theory . .	3.8×10^{-8}
J. J. Thomson, Recent Papers	3.34×10^{-8}
R. A. Fessenden, unpublished	2.79×10^{-8}

Taking into consideration the direction of the probable error of these determinations we obtain:

E. The diameter of the mercury atom is $2.75\ (\pm 0.2) 10^{-8}$ *cm.*

F. The ionic charge is $4\ (\pm 1) 10^{-10}$ *E. S. units.*

This ionic charge, "the natural unit of electricity," as Lodge aptly calls it (first calculated by Everett, and later, independently by Lodge) has long been considered as the cause of chemical action. This theory has been worked out in considerable detail by

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[1] Science, March 3, 1893.

[2] Molecular Physics, Frank. Inst., September, 1896.

[3] Nernst, from Clausius-Mossotti theory of dielectrics and from kinetic theory of gases; Risteen from surface tension of mercury by Kelvin's method; J. J. Thomson from ionic equivalent of gases; Fessenden from surface tension of mercury by Ostwald's method. All reduced to mercury atom.

Davy, Berzelius, Helmholtz, Arrhenius, Clausius, Ostwald, Nernst and others, with the result that we no longer consider "chemical force" as a separate force but merely as a particular manifestation of electric force.

The writer was able in 1890 to make a still further extension of the field of electric action by showing that there is no necessity for assuming a separate "cohesive force," but that the ionic charges fulfilled yet another distinct function, in that they are capable of accounting for the tensile strength, rigidity, Young's modulus and other mechanical properties of bodies. The field of physics, by the removal of these two forces, has thus been reduced to the consideration of the forces connected with electricity, magnetism, gravitation and inertia.

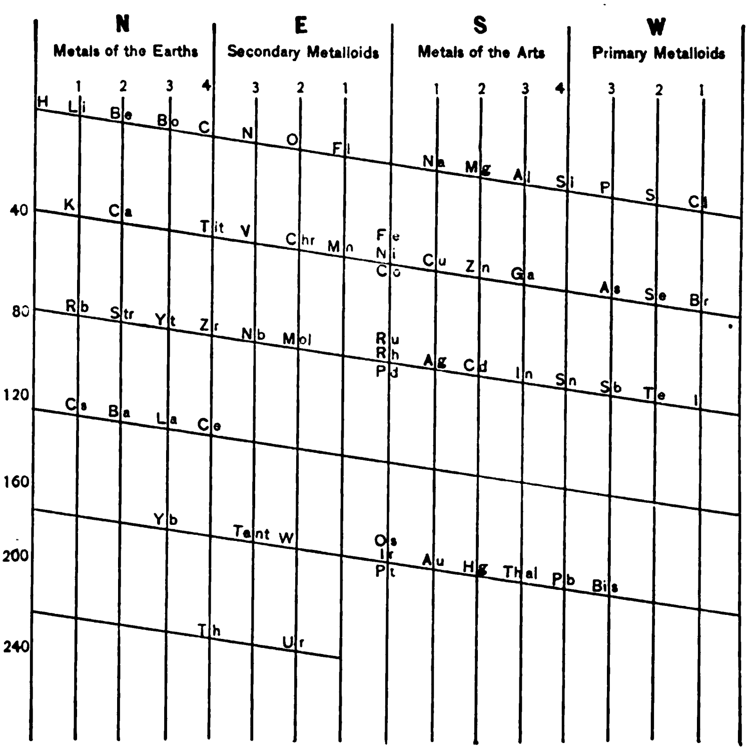

Fig. 2.

According to this theory,[1] the normal atom, in the neutral state, is really a doublet, *i. e.*, has two equal charges, one positive and one negative. Two such atoms when at a distance will exhibit no attraction for each other, but on being brought near one another will be attracted. Attraction under these circumstances we take to be cohesion. "If however, any third substance" (or as was pointed out later, any other molecular form of the same substance) "connects the outside parts of the atoms and so enables these parts to neutralize each other, we have chemical combination, and the two parts when separated show opposite charges, as at C."[1]

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[1] Not published till July, 1891, as the then novel idea of the atoms of metals having charges was considered by the editors to whom it was submitted as incompatible with the law that all charges must reside on the surface of conductors.

According to the electrostatic theory of cohesion, therefore,

G. Chemical action and cohesion are both manifestations of the same force, i. e., electrostatic attraction, under different conditions.

The resultant product of a cohesion effect is generally isotropic, whilst that of a chemical action (owing to the presence of atoms of different sizes or valencies) is crystalline, *i. e.*, "the atoms in one case may be grouped in any direction, and in the other they are only grouped about certain axes or planes."[2]

Taking the ionic charge as 4.10^{-10} E.S. units and the diameter of the silver atom as $2.42\ 10^{-8}$ cm., we find that the total quantity of electricity on a sheet of silver one atom thick and one cm. square is $6.8\ .10^5$ E.S. units. If for a first approximation we suppose the flux to be uniformly distributed we find for the attraction between two such sheets 3.4×10^7 dynes.

Considering a silver wire one mm. thick as made up of discs one mm. in diameter, we find, on this theory, that the attraction between two such discs is 270 kilogrammes.[3]

From Wertheim's experiments the tensile strength of a silver wire one mm. in diameter is 29 kilogrammes.

We have taken the flux density as uniform. We know that this cannot be true or the resistance to lateral shear would be very slight, in fact the substance would be a fluid. The amount of the attraction will depend upon the distribution of the charges, but we will find, on examination, that if we reject the idea of indefinitely large flux densities at any point, any conceivable distribution will give results of the same order of magnitude as those given above.

In this calculation we have neglected the kinetic repulsion. Our justification for this is that since the kinetic repulsion varies roughly as the cube of the distance between the surfaces of the atom, and the attraction varies as some power less than the square of that distance, the force required to stretch, shear or pull apart a body will, to a first approximation, be determined by the attractive force alone.

If the theory be correct and the disposition of the charges does not vary greatly for atoms of different elements, the mechanical properties should be functions of the size of the atoms, since the ionic charge is the same for all atoms. The following formulæ and tables show that such is the case :

A. Rigidity $= 28 \times 10^{12} \div (\text{atom. vol.})^2$
B. Young's Modulus $= 78 \times 10^{2} \div (\text{atom. vol.})^2$
C. Velocity of Sound $= \{78 \times 10^{12} \div (\text{atom. weight} \times \text{atom. vol.})\}^{\frac{1}{2}}$
D. Tensile Strength $=$ Absolute Temp. of Melt. pt. $\div$ (1.92 $\times$ atom. vol. $\frac{2}{3}$)
The last is for wires 1 mm. in diameter.

The elasticity of such substances as India-rubber, gelatine and ivory was investigated. It was found that the abnormally high value of Poisson's ratio for such substances was due to the fact that their elasticity, as measured in the ordinary way, bears no direct relation to the true elasticity of the material, but is a matter of configuration. It was shown[4] that such substances consist of two or more compounds, interpenetrating but not combining with one another. It was shown that the heating on stretching, the cooling on relaxing and the shortening when stretched and heated, followed as a natural consequence of the configuration. The different compounds were differentiated under the microscope and, in the case of India-rubber, by following Faraday's method, isolated chemically. The shape of the elasticity curve was predicted and the prediction was verified by the writer and by Professor Thurston.[1] Substances were made which had the shape of the elasticity curve, the property of polarizing light on being stretched, and the value of Poisson's ratio the same as rubber, but which on simple pressure could be differentiated into two substances, each of which had the usual form of elastic curve, now heated instead of cooling, on compression, and now had a value of Poisson's ratio nearly one-half of what it was previously. The behavior of these abnormal substances was thus shown not to conflict at any point with that called for by the electrostatic theory of cohesion.

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[1] Science, July 22, 1892. J. J. Thomson showed later that the proximity of a dielectric of greater capacity would have the same effect.

[2] Molecular Physics, Franklin Inst., Sept., 1896.

[3] Given as 45 kilogrammes in *Electrical World*, August 22, 1891, owing to a slightly different value having been taken for the size of the atom.

[4] Mol. Phys., Frank. Inst., September, 1896.

TABLE I.

Metal.	Atom. Vol.	Rigid. $\div 10^8$	Calc.	Young. M. $\div 10^8$	Calc.	Tens. str.	Ca
Iron.	7.1	750	550	2000	1560	65	74
Copper.	7.1	430	550	1220	1560	41	48
Platinum.	9.1					35	48
Zinc.	9.2	350	340	930	920	15.7	16
Silver.	10.2	280	270	740	750	29.6	29
Gold.	10.2	270	270	760	750	28.5	29
Aluminum.	10.4	250	260	680	690	18.	18
Cadmium.	13.		170	480	465		
Magnesium.[2]	14.	150	143	390	395	10.4	9
Tin.	16.3	136	100	420	295	3.4	5
Lead.	18.1	84	83	190	235	2.36	4

It was found that there existed a relation between the velocity of sound and the electric conductivity of the pure metals as follows:

H. If we take wires of two pure metals, having an equal number of molecules in the cross section, then the resistances of the wires will be proportional to the times taken by a sound wave to traverse them.

The supposition is here made that whilst, as regards specific heat, the pure metals behave as if there were only one atom in the molecule, as regards electricity they behave as having the number of atoms per molecule numerically equal to the valence. The grounds for this assumption may be questioned, the reasons for making it are given in another paper.[3]

The formula for the electric resistivity at 0° C. of any metal is thus:

$$\text{Resistivity} = 14.04 \times \text{atom. vol.} \times \sqrt{\text{atom. weight}} \times \text{valency}.$$

Table II. on opposite page shows the observed and calculated resistivities.

The tests on Zn, Sn and Pb were made on pressed wires.

It should be pointed out that the conductivities of two substances, which, as determined at the time when this relation was first published,[4] were too low, have since been found, by later experimenters working with pure materials, to be in agreement with the formula. I refer to aluminium and magnesium, whose resistivities were given as 2881 and 4162 respectively, but the former of which has been found by Richards and Thomson[1] to have the restivity 2370 for the pure material, and magnesium, which has been shown by Thurston[2] to have a resistivity of 3835. In the case of zinc, tin and lead it will be noted that through the absolute values are low, the ratios are correct. It is barely possible that the fact that these three materials were all tested in the form of squirted wires may have something to do with the discrepancy.

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[1] Science, May 1898.

[2] The tensile strength of magnesium was predicted, Science, July 22, 1892, and Thurston's tests, London Elect., June 5, 1896, showed it to agree with theory.

[3] Conduction and Insulation, Amer. Inst. E. E., March, 1898. [4] Science, July 22, 1892.

TABLE II.

Metal.	Atom. vol.	Atom.wt.½	Valency.	R. calc.	R. obs.	Observer.
Cu.	7.1	7.94	2	1583	1580	Mathiessen.
Ag.	10.2	10.39	1	1488	1488	"
Au.	10.2	14.05	1	2012	2036	"
Al.	10.5	5.2	3	2300	2370	Rich. & Thomp.
Mg.	14.	4.9	4	3852	3835	Thurston.
Zn.	9.4	8.09	4	4270	5566	Mathiessen.
Cad.	13.	10.58	4	7722	6720	Benoit.
Sn.	16.2	10.86	4	9880	13070	Mathiessen.
Pb.	18.2	14.38	4	14695	19420	"

This formula, whilst in some cases not entirely satisfactory (though in view of the fact that already in two cases the fault has been found to lie with the physical measurement and not with the formula, it may happen that the other exceptions may also fall into line) is of interest as linking together the phenomena of the conduction of sound, heat and electricity in wires. I have suggested elsewhere that the velocity with which heat and electricity travel in a wire may be that of sound. In the case of heat, the experiment might possibly be made, in spite of the very rapid logarithmic tailing down, due to the wire's heat capacity (hence similar to the case of an electric cable), by a thermocouple at one end of a short piece of gold or platinum, heated very suddenly at the other end by electric means. But in the case of the electric current, as Maxwell and Heavyside have pointed out, we know absolutely nothing of the velocity with which electricity travels in the wire. "It may be an inch an hour or it may be immensely great."[3]

The theory was applied to fluids. It was shown that whether a body is solid or fluid or a gas or vapor depends upon whether the fraction

$$\frac{\text{Cohesive force of atoms for one another} + \text{external force}}{\text{Kinetic repulsion} + \text{cohesive attraction for other atoms}}$$

is greater or less than unity. From this equation of state it will be seen that we can produce dissociation in two ways: by decreasing the numerator or increasing the denominator. If we increase the first term of the denominator we disassociate by heating, if we increase the second term by bringing the atoms, say of NaCl, in contact with other atoms, of say H_2O, we disassociate by solution. It is shown that this leads us to the idea of an "osmotic suction" instead of an "osmotic pressure," and that the change in the

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[1] Journal Franklin Institute, March, 1897.

[2] London Elect., June 5, 1896.

[3] Heavyside, Papers, II., p. 394.

"volume force" thus brought about accounts for the heating on solution, and other difficulties of the "osmotic pressure" theory. Some applications were made to the theory of electrolytic conduction.

The theory was also applied to gases, and it was pointed out that the constant a in Van der Waal's equation

$$(p + a/v^2)(v - b) = RT$$

should be a function of the volume b. That this is true is shown by the following table, the values being taken from Ostwald's "Outlines of General Chemistry,"

Substance.	$a \times 10{,}000$	$b \times 10{,}000$.
Dyethylamine	355	58
Ethyl. Acet.	348	55
Ether	324	57
Benzine	(438)	51
Ethyl. Form.	304	48
Chloroform	287	44
Acetone	273	44
Methyl. Acet.	248	39
Alcohol	236	37
Ethyl. Chlor.	227	40
CS_2	219	33
SO_2	123	24
NO_2	(74)	19

and a modified form of the equation, *i. e.*,

$$(p + c/b^{4/3})(v - b) = RT$$

was suggested, c having the same value for all gases.

Although received with so little favor at first that it was over a year before publication could be obtained, the theory seems now to be on a better footing. The electrification of separate sheets of mica, etc., is in its favor, and Ostwald pointed out in a letter to the writer, September 16, 1891, that some experiment of his along a similar line were of interest in this connection.[1]

About a year and a half after its first publication, it was shown by Chattock (Phil. Mag., Dec., 1892) that the piezo-electric and thermo-electric properties of crystals, and the dielectric strength of gases, could be accounted for along these lines.

In the case of these phenomena, since we are dealing with chemical compounds, and not simple elements, there is nothing to show that the effect is not merely a mechanical-chemical one, or that there does not exist a force of cohesion *in addition* to the force exerted by the electric charges which we know to be active in chemical action. But Chattock was led, in addition, independently, it appears, to the same conclusion as the writer, *i. e.*, that cohesion as well as chemical action is to be accounted for by the ionic charges.

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[1] "The electrostatic theory of cohesion is new to me, and interesting, in as much as, according to my experiments, the cleavage surfaces of almost all bodies appear to be electrically charged." A point which he raises, "For electrolytes there would be the question to answer, 'Why substances like alcohol, etc., show no ions?' whilst according to your theory, all elements have electric charges" is, I think, explained on taking into account the different ways in which the atoms are charged in chemical and cohesive effects.

MAGNETIC ROTATION OF LIGHT.

Both Kelvin and Maxwell, from a study of the phenomenon of the magnetic rotation of light, came to the conclusion that, to quote Maxwell, "at each point of the medium something exists of the nature of an angular velocity about an axis in the direction of the magnetic force."

I have never been able to persuade myself of the validity of the proof given, as, in spite of Maxwell's disclaimer, it seems to me that both proofs virtually imply a certain class of theories of light. If, indeed, it were proven that rotation is possible in vacuo, then we would be forced to conclude that some one of these light theories was true. But so far this has not been shown.

It is, I think, not difficult to see that the phenomenon may arise from a simple strain, and that an angular velocity is not necessitated. The rotation of a plane wave is reversed on reflection backwards through a suitably chosen medium, but the rotation of other forms of periodic motion may be doubled. Take a couple of wires of equal length, and string beads on them. Twist one into a right-handed spiral and the other into a left-handed spiral. Take any medium such that on stress it becomes a mass of right- or left-handed spirals, for example a lot of strands of wire rope, straightened and packed loosely side by side. In this condition, if equal forces be applied to right- and left-handed spirals, one will pass through the medium as fast as the other. Compress the bundle of strands, however. It will then turn to a mass of loosely packed right-handed spirals. Through this the right-handed spiral will now penetrate much faster than the left-handed. If, after passing through the medium, the wires strike a plate, and being bent back, are forced to reënter the medium and traverse it in the reverse direction, the right-handed spiral will still travel the faster, and its total gain will be twice that given by traversing the medium in the original direction.

So that the assumption that when we have a magnetic field we have an angular velocity about the axis of magnetic force really implies a theory of matter, and taken by itself, in the absence of any knowledge as to whether light can be rotated in a vacuum, is incapable of proving anything in regard to the nature of the magnetic field.

I have pointed out elsewhere[1] that "It might be supposed that the electro-magnetic rotation of light would give us a fourth equation. * * * Since the atoms carrying charges are vibrating in every direction, we have really an infinite number of electric currents, and it is obvious that when such a system is placed in a magnetic field the velocity will be greatest in a direction parallel to the current in the nearest part of the magnetizing coil, as if the motion be otherwise, work will be done in forcing the atoms to move in that direction. In either case we get only an equation between the electric and magnetic quantities which is implicitly contained in the first three equations."

This change in velocity when the ions are placed in a magnetic field has since been shown by Lorentz to be the cause of the Fiévez or Zeeman effect. It was, however, found that this ionic movement did not give results of the same order as those necessary for the explanation of magnetic rotation and the following theory (communicated to Professor Fitzgerald, December, 1898) was taken as being the most probable one.

In the electromagnetic wave the energy exists in two forms—electric and magnetic. Consider the electric displacement by itself. It produces, by itself, a magnetic force which is a maximum at the instant when the displacement is zero. So long as the energy is being transmitted without loss, the electric displacement and the electric force producing it are a maximum at the same instant. But suppose that there is absorption, then the voltage and displacement current will no longer be 90 degrees apart, but the angle will be less. The voltage may now be considered as split up into two components, one in phase with the displacement and one at 90 degrees from it, in point of time. Hence while, when there was no loss of energy, the plane of the electric displacement was merely rocked to and fro, there will now be a to and fro rocking due to the one component, together with a continuous twist due to the other component similarly with the magnetic component. In other words, whenever we have absorption, the light wave is rotatable when placed in a magnetic field. Some evidence in proof of this has already been obtained, and will be published later. Here it will be sufficient to point out that those substances which absorb most strongly, such as selenium, or copper oxide, give the greatest rotation, and that the rotation appears, in those cases so far examined, to be greatest at that point in the spectrum where the absorption is greatest.

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[1] Electrical World, May 18, 1895.

THERMO-ELECTRICITY.

A number of experiments were made to see if there was any evidence that thermo-electricity depended upon the energy of combination of the materials of the circuit, analogous to the discovery of Exner that the voltage of cells could be calculated from the heats of combination of their elements. It was found, after examining the records of a number of thermopiles, that in every case the total amount of energy which had been obtained from them was of the same order as that which would have been furnished by the heat of combination of the materials forming the couples. Two holes were then bored in a slab of antimony, and the two ends of a copper rod of ½ inch diameter were driven in them. The whole was then fixed to a steam pipe in constant use so that one end was in contact with the pipe and the other in the free air. After a year the combination was examined. It was found that at one junction there was a considerable quantity of violet colored alloy of copper and antimony. whilst at the other there were only traces. As, however, one of the contacts had gone bad, at some time unknown, it was impossible to make a quantitative estimation, and the experiment will be repeated at a favorable opportunity. It is possible that thermo-electric effects are intimately connected with the phenomenon of molecular diffusion in solids, discovered by Roberts-Austen, and so strikingly illustrated by his admirable experiments.

CONFIGURATION OF ATOM.

Since every atom is charged and the charge has the quality M/T, *i. e.*, current of inertia, we thus for the first time have a definite proof of the vortex nature of the atom. A considerable number of other points were investigated, more or less fully, but will be discussed elsewhere.

APPENDIX C.

NOMENCLATURE, SYMBOLS AND UNITS.

Mental, as well as mechanical processes, are vastly facilitated by the presentation of the material to be worked up in a form adapted to the machinery which is to be employed upon it. When we have stated our data in terms of suitable units, we have accomplished a sort of preparatory rough planing and surfacing of our facts, and as a result we may then proceed to make our deductions with much greater ease and with greater certainty.

In some respects our present systems of units are very imperfect. Aside from the fact that the duodecimal system would have been much preferable to the decimal, a great opportunity was lost when the meter was taken as a decimal part of the earth's circumference instead of being made equal to twice the distance passed over in the first second, by a body falling from rest, at sea level, at Paris, and in vacuo. This would have made the unit force the weight of unit mass, instead of $\frac{1}{981}$ of the weight of unit mass. We do not care whether a block of metal of one kilogramme mass has its side such and such a fraction of the earth's circumference or not, but it would be very convenient to know that 200 units of energy would give it a velocity of 20 units of length per second, and that unit work would be done in raising it through unit distance. True, the relation would not have held exactly over the whole earth, but neither is the cubic centimeter of water exactly equal in mass to the gramme (it being about two-tenths of one per cent. less than a gramme at ordinary temperatures), and the practical advantages of this latter approximate relation have not, to my knowledge, been disputed.

If the unit length had been taken as 981 of our present centimeters, the weight of unit mass would have been equal to the unit force to within two-tenths of one per cent. (*i. e.*, to within the same degree of accuracy with which a cubic centimeter of water at ordinary temperatures represents a gramme of mass), over practically all those portions of the earth where the mechanic arts have attained their highest development. At the equator this practical unit of work would be slightly deficient in size, a circumstance which some slight acquaintance with the inhabitants of tropical countries leads me to believe would not be highly resented by them.

In our electric and magnetic systems especially, the state of affairs is very deplorable. In addition to the two necessary systems of units, *i. e.*, the electrostatic and the electromagnetic, we have a third one, which is absolutely unnecessary, *i. e.*, the so-called practical system.

To express the relations between the units of this system and of the electromagnetic system a large number of constants of various sizes are employed. This creates great consequent confusion and annoyance.

Again, as Heavyside has pointed out, the definition of unit quantity of electricity and unit quantity of magnetism by the equations

$$\frac{QQ'}{\kappa L^2} = \text{Force}, \qquad \frac{PP'}{\mu L^2} = \text{Force},$$

instead of by the equations

$$\frac{QQ'}{4\pi\kappa L^2} = \text{Force}, \qquad \frac{PP'}{4\pi\mu L^2} = \text{Force},$$

has the effect of introducing the very objectionable constant 4π into many of our most frequently used and most important formulæ.

Another point, which is equally important, is that the use of the present defining equations, given above, necessitates the employment of units of electric and magnetic flux in addition to the units of quantity of electricity and quantity of magnetism, the units of flux being of the same nature as the units of quantity, but being only $\frac{1}{4}\pi$ times the size of the former. This again gives rise to two distinct sets of coefficients, differing from each other in the same way.

On examination it will be found that the number of totally unnecessary electric and magnetic quantities thus introduced is not less than 46 in number, and, moreover, we must have the same number of additional unnecessary symbols.

To take an example, If we wish to know what electrostatic flux corresponds to 3 coulombs, we must

1. Divide by 10 to reduce to absolute electromagnetic units.
2. Multiply by 3.10^{10} to reduce to electrostatic units.

3. Divide by 4π to reduce to units of flux, the result being,

$$3 \text{ coulombs} = 23873 + 10^4 \text{ electrostatic lines.}$$

It seems[1] that there is a very strong feeling amongst the electricians and electrical engineers of this and other countries in favor of a return to absolute units and the abandonment of the practical system. Although some of the practical units have been legalized there is nothing to prevent our giving names to the absolute units and using them exclusively for practical work.

There remains, however, the 4π difficulty. Heavyside has proposed to get rid of this constant, or rather, to transfer the "eruption of 4πs" to a place where it will do no harm, by making the unit of current $1/\sqrt{4\pi}$, and the unit of voltage $\sqrt{4\pi}$ times as large as the present units.

But whilst this would undoubtedly have been the best way, if it could have been done in 1873, it is now not practicable. It necessitates changing the ampère, coulomb, volt, ohm, farad and henry. Most of these have been legalized, and a very large sum has been locked up in standards. Were any attempt made to change the present legal units, the government would probably, and justly, point out that it is only a few years since the present units were recommended for adoption, and request its petitioners to excuse the taking of any further action until some evidence was furnished that the proposed alterations were likely to meet with a permanent approval.

I have found, however, that by a great piece of good fortune there exists a way in which the 4π can be got rid of without disturbing any of the legal units. It consists in shifting the 4π from the unit of quantity into the coefficient μ.

From the equation

$$\text{curl}\,(QP) = \text{Work}/T$$

we see that, there being but one curl, we have but one 4π. If we keep our units of mass, length and time unchanged, we can get rid of the 4π in three ways: 1. By dividing up the 4π between Q and P, *i. e.*, making each $1/\sqrt{4\pi}$ times as large as before. This was suggested by Heavyside. 2. By putting the 4π over on the other side, *i. e.*, making the unit of work 4π times as large as before. This is impracticable. 3. By making one of the two quantities, Q or P, $1/4\pi$ times as large as before.

The third method is the practical one. We cannot change the coulomb, but we can take the unit quantity of magnetism as equal to the present line, and the unit of difference of magnetic potential as equal to the current turn, without creating any appreciable disturbance with the existing order of things. Neither of these quantities has been defined by law, and as a matter of fact the ampère turn is already used by designers as a practical unit of gilbertance.

We now have

$$dQ/dT = \text{voltance}$$
$$dP/dT = \text{gilbertance}$$

and have, therefore, now no use for the "flux" as distinct from the "quantity." We have, therefore, no use for the quantities, specific inductive capacity and permeability, *i. e.*, for the ratios

electric flux density ÷ volts per cm.
magnetic flux density ÷ gilberts per cm.

whereas we still have use for the ratios

quantity of electricity per sq. cm. ÷ volts per cm.
quantity of magnetism per sq. cm. ÷ gilberts per cm.

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[1] See in this connection the following articles: Editorial, Electrical World, June 29, 1899. Blondel, Electrical World, July 29, 1899, and the writer, Electromagnetic Design. Franklin Inst., April, 1899.

No names have been given to these quantities, though the ratio
intrinsic quantity of magnetism per sq. cm. ÷ gilbert
has been termed the "susceptibility."

We might use the present terms, specific inductive capacity and
these latter ratios. But it is always objectionable to give an old n
and I therefore suggest that the ratios

$$Q \text{ per sq. cm.} \div F, \text{ and } P \text{ per sq. cm.} \div H$$

be termed the "capity" and "permity" respectively. The simila
indicate their relationship to the quantities specific inductive capac
and we may retain the symbols κ and μ for the quantities capity an
tioned above, we have introduced the 4π into the constant μ. The
is then 4π, whereas the permeability for vacuo (or air. It should
given for vacuo and not for air) is now unity. The value of the elec
the same as before, only, as we are now dealing with a different thi
quantity ÷ voltivity instead of flux ÷ voltivity, we now speak of the
as being $\frac{1}{4}\pi$ where before we spoke of the specific inductive capac
unity.

It will be seen that we have put the 4π where it will do no harm.
have now $\mu = 1846$ where before we had $\mu = 147$ is a matter of no
one uses published tables of permeability for designing, for no two sam
the same value and as each sample comes into the factory it must have
termined just as if no other sample of iron had ever existed. As a matt
our data with respect to slightly magnetic substances has been publishe
tables of susceptibility and not of permeability.

The same remarks apply to capacity. No one would think of using
ties for making cables or condensers. Very little is known of the true capaci
as this constant is affected to a very great extent by traces of moisture.
been able to reduce Hopkinson's figures for oils very materially by using
carefully dried. As another example, the capacity of india rubber is giv
2.80, and by Salford and Halman as 3.7 whilst cable manufacturers, wh
treatment to eliminate water, will furnish it as low as 2. No one would t
of using published tables for any practical or theoretical work of importanc
cables or condensers a sample is always made up first and the capacity c
that.

I give below a table showing the relation between the present and sugges
formulæ.

Present Units.	*Suggested Units.*
Coulomb	No change.
Ampère	"
Volt	"
Ohm	"
Farad	"
Erg. Joule, Watt	"
Specific Ind. Capacity.	No change, but we have now no use for t specific inductive capacity, using capity inste
Permeability.	$(4\pi)^2$ times as large. But having now this quantity we use the permity, which is 4π
Quantity of Magnetism.	$\frac{1}{4}\pi$ times as large as before, *i. e.*, equals pr
Difference of Magnetic Potent.	4π times as large as before.

Present Equations.	*Suggested Equations.*
$\frac{QQ'}{\kappa L^2} = \frac{PP'}{\mu L^2} = \text{Force},$	$\frac{QQ'}{4\pi\kappa L^2} = \frac{PP'}{4\pi\mu L^2} = \text{Force}.$
$E = 4\pi\frac{dP}{dT},\ G = 4\pi\frac{dQ}{dT},$	$E = \frac{dP}{dT},\ G = \frac{dQ}{dT},$
$\frac{D^2}{8\pi\kappa} = \frac{B^2}{8\pi\mu} = \text{Energy per c.c.}$	$\frac{D^2}{2\kappa} = \frac{B^2}{2\mu} = \text{Energy per c.c.}$

I gave also a complete table of the quantities, symbols and units which would be necessary under the new system. It will be noted that 46 quantities, whose existence is now necessitated, owing to the present distinction between quantity and flux, have been dropped, as not required with the new system.

No importance is attached to the *names* suggested. They can, in most cases, be improved upon. Stress is however laid upon the symbols proposed, which it is thought will not readily be bettered. For vector quantities in the electric, magnetic and electromagnetic systems, block or script letters may be used, and ordinary type for scalers. Where, as in ælotropic substances, the capity, permity or conductivity has direction, cursives may be used. For the mechanical quantities, gravity and heat, italics are used. Vector quantities here may be either block or script. For radiation, German type is used.

The terminations have the following meanings.

a, Absolute.
ma, Absolute electromagnetic.
sta, Absolute electrostatic.
l, amount per cm.
r, amount per sq. cm.
v, amount per cubic cm.
t, amount per second.
rt, amount per sec. per sec.
st, amount per sec. per sec. per sec.

The syllable *al* denotes extrinsic quantity.

The syllable *id*, denotes intrinsic quatity.

It will be noted that all the regular uncials are free for use for other purposes, as also the letters d, δ, Δ and X, Y, Z.

The idea of using terminations in this manner is of course not new. Quite recently, since the above was written, the matter has been brought forward again in a very interesting paper by M. Hospitalier.

The changes suggested can be made without interfering with any of the legal units, without rendering useless any tables used in practical work, and will merely render it necessary to alter the values of the electric constants of the few substances whose capacities we know accurately. In fact, even this is not necessary, for since we have made no change in the unit of capacity, but have merely introduced a new quantity, the capity as being more convenient, those who choose may still employ the present term, specific inductive capacity, and use the present values.

It may be mentioned that if, at some future time, we shall revise our units, taking the mass of a cubic cm. of the ether as the unit of mass, and twice the distance passed over in the first second by a body falling, from rest, in vacuo, at Paris, at sea level, as our new meter, we may use a system of symbols and names naturally provided for us.

For, it will be noted that all physical quantities have their qualities in the form

$$\frac{M^v L^z}{T^y L^x}$$

where x, y and z may have the value 1, 2 or 3, and v the value 0 or 1. Consequently all physical quantities can be symbolized by using the one single letter *H* with different lengths of arms.

Thus, *F* represents electric current per sq. cm.

/' represents energy per cubic cm.

H represents stress per unit area.

For writing, we may use / for 1, (for 2, and) for 3.

Different fonts of type can be used to distinguish between electric, magnetic and other quantities. At present this suggestion would hardly meet with approval from the printers

Similarly, if we take *m* for *M*, *t* for *T*, *rt* for T^2, *st* for T^3, *a* for *L*, *e* for L^2, *i* or *y* for L^3, *u* for two *L*'s in the same direction and *o* or *n* for zero for each possible physical quantity we get a corresponding word.

Thus, Elmore would represent electric current.

Mury would represent energy per c. c.

Marte would represent stress per unit area.

The matter is, however, for the present at least, one of curiosity rather than of practical importance.

ADDENDA.

The coefficient η, referred to several times, is the coefficient in Steinmetz's formula

$$\text{Hysteresis loss per c. c. per cycle} = \eta B^{1\cdot6}.$$

Having these relations between H, B, μ, η and the elasticity, we are in a position to develop the quantitative mathematical side much further. For example, we may substitute in a number of terms in the Lagrangian function treatment of J. J. Thomson (Dynam. App. to Phys. and Chem.), and obtain a much more complete idea of the relation between elastic strain and magnetization.

The qualitative formula for temperature, given above, *i. e.*, energy per atom, is for monatomic molecules. It is better given as energy per molecule.

The statement that μ varies with H whilst κ does not vary with F is not to be understood as meaning that change of F makes *no* change of κ, but that it is not of the nature required to make κ a compliancy. It will, however, be seen that κ should vary inversely as F^2, for $M/L^3 = T^2/L^2 \times ML/T^2$. This phenomenon has as yet not been discovered.

ELECTRIC.

Name.	Symbol.	Equation.	Suggested Name for Absolute Electro-Magnetic Unit.	Size of Suggested Absolute Unit.
Quantity	Q	$Q^2/4\pi\kappa L^2 = \Phi$	Coulma	Same as present
Surface Density	D	$D \times \text{area} = Q$	Coulmar	"
Volume Density	C	$C \times \text{volume} = Q$	Coulmav	" (Note)
Capity	κ	$\kappa F = D$		"
[illegible]	λ	$F/\lambda = D$		"
Capance	K	$EK = Q$	Faradma	"
Elastance	Λ	$E/\Lambda = Q$	Darafma	"
Voltance	E	$EQ = \Psi$	Volma	"
Voltivity	F	$E \div \text{length} = F$	Volmal	"
...idity	Q_i	$Q_i + Q_e = Q$	Coulidma	"
...ality	Q_e	$Q_e = EK_e$	Coulalma	"
...y	κ_i	$\kappa_i + \kappa_e = \kappa$		"
...lity	λ_i	$\lambda_i = 1/\kappa_i$		"
...ance	K_i	$K_i + K_e = K$	Farididma	"
...idance	Λ_i	$\Lambda_i = 1/K_i$	Darafidma	"
...ality	κ_e	κ_e is κ of ether		"
...nality	λ_e	λ_e is λ of ether		"
Capalance	K_e	K_e is K of ether	Faradalma	"
Elastalance	Λ_e	Λ_e is Λ of ether	Darafalma	"
Temperature Coefficient	κ_E, λ_E			

attached to the names suggested, these being given merely for the purpose of illustrating the system. The
nd for the terms "absolute unit," "absolute electro-magnetic unit" and "absolute electrostatic unit" respec-
-, -t, -rt, -st stand for quantity per cm., per sq. cm., per c. c., per second, per sec. per sec, and per sec. per sec.
able "id" is used for "intrinsic quantity" and the syllable "al" for "extrinsic quantity."

MAGNETIC.

me.	Symbol.	Equation.	Suggested Name for Absolute Electro-Magnetic Unit.	Size of Suggested Absolute Unit.
[illegible]	P	$P^2/4\pi\mu L^2 = \phi$	Gausma	$1/4\pi$ times present *i. e.*, equal to present magnetic line
ce Density	B	$B \times \text{area} = P$	Gausmar	$1/4\pi$ times present
ume Density	A	$A \times \text{volume} = P$	Gausmav	$1/4\pi$ times present
Permity	μ	$\mu H = B$		$(4\pi)^2$ times present (Note 2)
Reluctity	ν	$H/\nu = B$		$1/(4\pi)^2$ " "
Permance	M	$GM = P$	Rema	$(4\pi)^2$ " "
Reluctance	N	$G/N = P$	Oersma	$1/(4\pi)^2$ " "
Gilbertance	G	$GP = \psi$	Gilma	4π times present *i. e.*, equal to amma turn
Gilbertivity	H	$G \div \text{length} = H$	Gilmal	4π times present
Quantidity	P_i	$P_i + P_e = P$	Gausioma	$1/4\pi$ times present
Quantality	P_e	$P_e = GM_e$	Gausalma	$1/4\pi$ times present
Permidity	μ_i	$\mu_i + \mu_e = \mu$		$(4\pi)^2$ times present
Reluctidity	ν_i	$\nu_i = 1/\mu_i$		$1/(4\pi)^2$ " "
Permidance	M_i	$M_i + M_e = M$	Remida	$(4\pi)^2$ " "
Reluctidance	N_i	$N_i = 1/M_i$	Oersidma	$1/(4\pi)^2$ " "
Permality	μ_e	μ_e is μ of ether		$(4\pi)^2$ " "
Reluctality	ν_e	ν_e is ν of ether		$1/(4\pi)^2$ " "
Permalance	M_e	M_e is M of ether	Remalma	$(4\pi)^2$ " "
Reluctance	N_e	N_e'' is N of ether	Oersalma	$1/(4\pi)^2$ " "
Hysteresis Coefficient	η		Hergav.	
Temperature Coefficient	μ_E, ν_E			

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2. In our present system, the permity of air is $1/4\pi$.

ELECTRIC.

[illegible]	*Name.*	*Symbol.*	*Equation.*	*Suggested Name for Absolute Electro-Magnetic Unit.*	*Size of Suggested Absolute Unit.*
[illegible]	Quantity	Q	$Q^2/4\pi\kappa L^2 = \Phi$	Coulma	Same as present
...n.	Surface Density	D	$D \times \text{area} = Q$	Coulmar	"
[illegible]	Volume Density	C	$C \times \text{volume} = Q$	Coulmav	"
...oefficient	Capi...	κ	$\kappa F = D$		" (Note)
... do.	[illegible]	λ	$F/\lambda = D$		"
...ity Factor	[illegible]	K	$EK = Q$	Faradma	"
...ip. of do.	[illegible]	Λ	$E/\Lambda = Q$	Darafma	"
Potential Differen...	[illegible]	E	$EQ = \Psi$	Volma	"
Potential Diffe...	[illegible]	F	$E \div \text{length} = F$	Volmal	"
Intrins...	[illegible]	Q_i	$Q_i + Q_e = Q$	Coulidma	"
Extr...	...nsity	Q_e	$Q_e = EK_e$	Coulalma	"
[illegible]	...pidity	κ_i	$\kappa_i + \kappa_e = \kappa$		"
[illegible]	Recusidity	λ_i	$\lambda_i = 1/\kappa_i$		"
[illegible]	Capidance	K_i	$K_i + K_e = K$	Farididma	"
[illegible]	Recusidance	Λ_i	$\Lambda_i = 1/K_i$	Darafidma	"
...efficient	Capality	κ_e	κ_e is κ of ether		"
[illegible]	Recusality	λ_e	λ_e is λ of ether		"
...ity Factor	Capal...	K_e	K_e is K of ether	Faradalma	"
... do.	R...	Λ_e	Λ_e is Λ of ether	Darafalma	"
... for degree C.	[illegible]	κ_E, λ_E			

...names suggested, these being given merely for the purpose of illustrating the system. The ... terms "absolute unit," "absolute electro-magnetic unit" and "absolute electrostatic unit" respec- ... -rt, -st stand for quantity per cm., per sq. cm., per c. c., per second, per sec. per sec. and per sec. per sec. ...d" is used for "intrinsic quantity" and the syllable "al" for "extrinsic quantity."

MAGNETIC.

Name.	*Symbol.*	*Equation.*	*Suggested Name for Absolute Electro-Magnetic Unit.*	*Size of Suggested Absolute Unit.*
uantity	P	$P^2/4\pi\mu L^2=\phi$	Gausma	$1/4\pi$ times present *i. e.*, equal to present magnetic line
Surface Density	B	$B\times$ area $=P$	Gausmar	$1/4\pi$ times present
Volume Density	A	$A\times$ volume $=P$	Gausmav	$1/4\pi$ times present
Permity	μ	$\mu H=B$		$(4\pi)^2$ times present (Note 2)
Reluctity	ν	$H/\nu=B$		$1/(4\pi)^2$ " "
Permance	M	$GM=P$	Rema	$(4\pi)^2$ " "
Reluctance	N	$G/N=P$	Oersma	$1/(4\pi)^2$ " "
Gilbertance	G	$GP=\psi$	Gilma	4π times present *i. e.*, equal to amma turn
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Quantidity	P_i	$P_i+P_e=P$	Gausioma	$1/4\pi$ times present
Quantality	P_e	$P_e=GM_e$	Gausalma	$1/4\pi$ times present
Permidity	μ_i	$\mu_i+\mu_e=\mu$		$(4\pi)^2$ times present
Reluctidity	ν_i	$\nu_i=1/\mu_i$		$1/(4\pi)^2$ " "
Permidance	M_i	$M_i+M_e=M$	Remida	$(4\pi)^2$ " "
Reluctidance	N_i	$N_i=1/M_i$	Oersidma	$1/(4\pi)^2$ " "
Permality	μ_e	μ_e is μ of ether		$(4\pi)^2$ " "
Reluctality	ν_e	ν_e is ν of ether		$1/(4\pi)^2$ " "
Permalance	M_e	M_e is M of ether	Remalma	$(4\pi)^2$ " "
Reluctance	N_e	N_e is N of ether	Oersalma	$1/(4\pi)^2$ " "
Hysteresis Coefficient	η		Hergav.	
Temperature Coefficient	μ_E, ν_E			

---

2. In our present system, the permity of air is $1/4\pi$.

ELECTRIC.

Nature.	*Name.*	*Symbol.*	*Equation.*	*Suggested Name for Absolute Electro-Magnetic Unit.*	*Size of Suggested Absolute Unit.*
Quantity	Quantity	Q	$Q^2/4\pi\kappa L^2 = \Phi$	Coulma	Same as present
Quantity per sq. cm.	Surface Density	D	$D \times$ area $= Q$	Coulmar	"
Quantity per c. c.	Volume Density	C	$C \times$ volume $= Q$	Coulmav	"
Quantity Coefficient	Capity	κ	$\kappa F = D$		" (Note)
Recip. of do.	Recusity	λ	$F/\lambda = D$		"
Quantity Factor	Capance	K	$EK = Q$	Faradma	"
Recip. of do.	Recusance	Λ	$E/\Lambda = Q$	Darafma	"
Potential Difference	Voltance	E	$EQ = \Psi$	Volma	"
Potential Difference per cm.	Voltivity	F	$E \div$ length $= F$	Volmal	"
Intrinsic Quantity	Quantidity	Q_i	$Q_i + Q_e = Q$	Coulidma	"
Extrinsic Quantity	Quantality	Q_e	$Q_e = EK_e$	Coulalma	"
Intrinsic Quantity Coefficient	Capidity	κ_i	$\kappa_i + \kappa_e = \kappa$		"
Recip. of do.	Recusidity	λ_i	$\lambda_i = 1/\kappa_i$		"
Intrinsic Quantity Factor	Capidance	K_i	$K_i + K_e = K$	Farididma	"
Recip. of do.	Recusidance	Λ_i	$\Lambda_i = 1/K_i$	Darafidma	"
Extrinsic Quantity Coefficient	Capality	κ_e	κ_e is κ of ether		"
Recip. of do.	Recusality	λ_e	λ_e is λ of ether		"
Extrinsic Quantity Factor	Capalance	K_e	K_e is K of ether	Faradalma	"
Recip. of do.	Recusalance	Λ_e	Λ_e is Λ of ether	Darafalma	"
Increase per degree C	Temperature Coefficient	κ_E, λ_E			

NOTE.—No importance is attached to the names suggested, these being given merely for the purpose of illustrating the system. The terminations -a, -ma and -sta stand for the terms "absolute unit," "absolute electro-magnetic unit" and "absolute electrostatic unit" respectively. The terminations -l, -r, -v, -t, -rt, -st stand for quantity per cm., per sq. cm., per c. c., per second, per sec. per sec. and per sec. per sec. per sec. respectively. The syllable "id" is used for "intrinsic quantity" and the syllable "al" for "extrinsic quantity."

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Name.	*Symbol.*	*Equation.*	*Suggested Name for Absolute Electro-Magnetic Unit.*	*Size of Suggested Absolute Unit.*
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Volume Density	A	$A\times$ volume $=P$	Gausmav	$1/4\pi$ times present
Permity	μ	$\mu H=B$		$(4\pi)^2$ times present (Note 2)
Reluctity	ν	$H/\nu=B$		$1/(4\pi)^2$ " "
Permance	M	$GM=P$	Rema	$(4\pi)^2$ " "
Reluctance	N	$G/N=P$	Oersma	$1/(4\pi)^2$ " "
Gilbertance	G	$GP=\psi$	Gilma	4π times present *i. e.*, equal to amma turn
Gilbertivity	H	$G\div$ length $=H$	Gilmal	4π times present
Quantidity	P_i	$P_i+P_e=P$	Gausioma	$1/4\pi$ times present
Quantality	P_e	$P_e=GM_e$	Gausalma	$1/4\pi$ times present
Permidity	μ_i	$\mu_i+\mu_e=\mu$		$(4\pi)^2$ times present
Reluctidity	ν_i	$\nu_i=1/\mu_i$		$1/(4\pi)^2$ " "
Permidance	M_i	$M_i+M_e=M$	Remida	$(4\pi)^2$ " "
Reluctidance	N_i	$N_i=1/M_i$	Oersidma	$1/(4\pi)^{\iota}$ " "
Permality	μ_e	μ_e is μ of ether		$(4\pi)^2$ " "
Reluctality	ν_e	ν_e is ν of ether		$1/(4\pi)^2$ " "
Permalance	M_e	M_e is M of ether	Remalma	$(4\pi)^2$ " "
Reluctance	N_e	$N_e^{\shortparallel}$ is N of ether	Oersalma	$1/(4\pi)^2$ " "
Hysteresis Coefficient	η		Hergav.	
Temperature Coefficient	μ_E, ν_E			

2. In our present system, the permity of air is $1/4\pi$.

ELECTRO-MAGNETIC.

Nature.	*Name.*	*Symbol.*	*Equation.*	*Suggested Name for Absolute Electro-Mag. Unit.*	*Size of Suggested Absolute Unit.*
Quantity of Electricity per sec.	Current	I	$I = dQ/dT$	Amperma	Same as present.
Q'ntity of Electricity per sq. cm. per sec.	Current Density	J	$J = I \div \text{area}$	Ampermar	"
Quantity of Magnetism per sec.	Flow	U	$U = dP/dT$	Weberma	$1/4\pi$ times present size *i. e.*, equal to present line per sec.
Q'ntity of Magnetism per sq. cm. per sec.	Flow Density	V	$V = U \div \text{area}$	Webermar	$1/4\pi$ times present size.
Conductivity	Conductivity	σ	$F\sigma = J$		No change.
Recip. of do.	Resistivity	ρ	$F/\rho = J$		"
Conductance	Conductance	S	$ES = I$	Moma	"
Recip. of do.	Resistance	R	$E/R = I$	Ohma	"
Quantity of Electricity × turns					"
Quantity of Magnetism × turns	Linkage	T	$T = P \times \text{turns}$	Maxawelma	"
Linkage ÷ current	Inductance	L	$L = T \div I$	Henryma	"
Imaginary Resistance due to Capance	Electric Reactance	R_K	$R_K = 1 \div K\omega$	Ohmak	"
Recip. of do.	Electric Permittance	S_K	$S_K = K\omega$	Momak	"
Imaginary Resistance due to Inductance	Magnetic Reactance	R_M	$R_M = L\omega$	Ohmam	"
Recip. of do.	Magnetic Permittance	S_M	$S_M = 1 \div L\omega$	Momam	"
Total Imaginary Resistance	Reactance	R_J	$R_J = L\omega + 1/K\omega$	Ohmaj	"
Recip. of do.	Permittance	S_J	$S_J = K\omega/(KL\omega^2 + 1)$	Momaj	"
Apparent Resistance	Impedance	R_v	$R_v = \sqrt{R^2 + (L\omega - 1/K\omega)^2}$	Ohman	"
Recip. of do.	Admittance	S_v	$S_v = 1/\sqrt{R^2 + (L\omega + 1/K\omega)^2}$	Moman	"
Increase per degree C.	Temperature Coeff.	σ_E, ρ_E			

HEAT.

Nature.	*Name.*	*Symbol.*	*Equation.*	*Unit.*
Quantity	Heat	Q	$Q = KE$	Erg.
Quantity per cc.	Heat Density	C	$C = Q \div V$	Ergav
Quantity Coefficient	Specific Heat	k	$kEV = Q$	
Quantity Factor	Heat Capacitance	K	$k \times V = Q$	
Potential Difference	Temperature Difference	E	$Ek = Q$	Degree
Potential Difference per cm.	Temperature Gradient	F	$E \div L = F$	Degreel
Quantity per sec.	Flux	I	$I = Q/T$	Ergat
Quantity per sq. cm.	Flux Density	J	$J = I \div A$	Ergart
Flux Coefficient	Impartivity	s	$Fs = J$	
Recip of do.	Morativity	r	$F/r = J$	
Flux Factor	Impartance	S	$ES = I$	Foura
Recip of do.	Moratance	R	$E/R = I$	Ruofa
Diffusion Coefficient	Diffusivity	s_E	$s_E = s \div k$	
Quantity Coefficient Constant Volume	Specific Heat, Constant Volume	k_v		
Quantity Coefficient Constant Pressure	Specific Heat, Constant Pressure	k_p		
Critical Constants	Critical Temperature	E_c		
	Critical Pressure	Π_c		
	Critical Volume	V_c		
Coefficient of	Linear Expansion	L_E		
" "	Surface "	A_E		
" "	Volume "	V_E		

RADIATION.

Nature.	*Name.*	*Symbol.*	*Equation.*	*Unit.*
Quantity of Radiation Emitted	Radiance	$\mathfrak{Q}$	$\mathfrak{g}\mathfrak{Q} = \mathfrak{P}$	Erg.
Quantity per sq. cm.	Radivity	$\mathfrak{D}$	$\mathfrak{D} \times \text{area} = \mathfrak{Q}$	Ergar
Quantity of Light Emitted	Lumenescence	$\mathfrak{P}$	$\mathfrak{P} = \mathfrak{g}\mathfrak{Q}$	
Quantity per sq. cm.	Lumenosity	$\mathfrak{B}$	$\mathfrak{B} \times \text{area} = \mathfrak{P}$	
Radiance per Degree C.	Emittance	$\mathfrak{K}$	$\mathfrak{K}\mathfrak{E} = \mathfrak{Q}$	
Radiance per sq. cm.	Emissivity	$\mathfrak{k}$	$\mathfrak{k}\mathfrak{E} = \mathfrak{D}$	
Lumenance per Degree C.	Lumenance	$\mathfrak{M}$	$\mathfrak{M}\mathfrak{E} = \mathfrak{P}$	
Lumenance per sq. cm. per Degree C.	Lumenity	$\mathfrak{m}$	$\mathfrak{m}\mathfrak{E} = \mathfrak{B}$	
Radiance Received	Receptance	$\mathfrak{q}$	$\mathfrak{e}\mathfrak{q} = \mathfrak{J}$	
Radiance per sq. cm.	Receptivity	$\mathfrak{d}$	$\mathfrak{d} \times \text{area} = \mathfrak{q}$	
Lumenescence Received	Luxance	$\mathfrak{p}$	$\mathfrak{f}\mathfrak{p} = \mathfrak{A}$	
Lumenescence per sq. cm.	Luxity	$\mathfrak{b}$	$\mathfrak{b} \times \text{area} = \mathfrak{p}$	
Radiance Absorbed	Absorbance	$\mathfrak{J}$	$\mathfrak{J} = \mathfrak{e}\mathfrak{q}$	
Radiance per sq. cm.	Absorbity	$\mathfrak{I}$	$\mathfrak{I} \times \text{area} = \mathfrak{J}$	
Lumenescence Absorbed	Illumance	$\mathfrak{A}$	$\mathfrak{A} = \mathfrak{f}\mathfrak{p}$	
Lumenescence per sq. cm.	Illumity	$\mathfrak{V}$	$\mathfrak{V} \times \text{area} = \mathfrak{A}$	
Absorbity ÷ Receptivity	Absorbance Efficiency	$\mathfrak{e}$	$\mathfrak{e} = \mathfrak{I} \div \mathfrak{d}$	
Illumity ÷ Luxity	Luxance Efficiency	$\mathfrak{f}$	$\mathfrak{f} = \mathfrak{V} \div \mathfrak{b}$	
Lumenity ÷ Radivity	Luminous Efficiency	$\mathfrak{g}$	$\mathfrak{g} = \mathfrak{B} + \mathfrak{P}$	

GRAVITY.

Nature.	*Symbol.*	*Nature.*	*Symbol.*
Gravity	G	Acceleration of Gravity	i
Quantity Coefficient	γ	Ponderate Acceleration (note)	g

MECHANICS.

Nature.	*Symbol*	*Nature.*	*Symbol.*
Mass	M	Angular Velocity	ω
Length	L	Work	Ψ
Area	R	Work per cm. or Force	Φ
Volume	V	Work per sq. cm. Surface Tension	Σ
Density	ς	Work per c. c. or Pressure	Ξ
Velocity	v	Work per second	Π
Periodicity	τ	Force per cm.	Γ
Acceleration	a	Young's Modulus	ζ
Plane Angle	φ, θ	Rigidity	ξ
Solid Angle	Ω	Tensile strength	χ

NOTE.—*I. e.*, acceleration due to gravity plus that due to rotation of earth on its axis, around the sun, etc.

THERMODYNAMIC NOTES (NO. 10): REVISION OF SOME THERMAL DATA CONCERNING BENZENE.[1]

K. TSURUTA.

PROFESSOR S. YOUNG[2] has given full measurements on vapor pressure (p) and specific volumes (v_l liquid and v_g vapor) of benzene in its saturated states. The values of $\frac{dp}{dT}$ were found from Biot's formula used by him, and the heat of vaporization (r or r_{lg}) was calculated, the results being given in the following table, in which for reference, some of the original data are reproduced.[3]

HEAT OF VAPORIZATION r (ACCORDING TO PROFESSOR S. YOUNG).

t	v_l	v_g/v_l	p	r	t	v_l	v_g/v_l	p	r
	c.c.		m.m.	Cal.		c.c.		m.m.	Cal.
80°C.	1.2278	298.0	753.62	95.34	180°C.	1.4480	27.90	7625.2	74.72
90	1.2436	225.0	1016.1	94.45	190	1.4798	22.70	9049.4	71.89
100	1.2615	169.0	1344.3	91.60	200	1.5139	18.65	10663.0	69.01
110	1.2806	129.0	1748.2	89.02	210	1.5546	15.23	12482.0	65.75
120	1.3000	100.0	2238.1	86.41	220	1.5986	12.47	14526.0	62.23
130	1.3214	79.4	2824.9	84.78	230	1.6487	10.15	16815.0	58.44
140	1.3440	62.7 63.60	3520.0	82.06 83.25	240	1.7091	8.21	19369.0	54.13
150	1.3680	50.5 50.90	4334.8	80.17 80.82	250	1.7827	6.55	22214.0	49.36
160	1.3918	42.10	5281.9	80.31	260	1.8770	5.14	25376.0	43.79
170	1.4198	34.30	6374.1	78.13	270	2.0063	3.88	28885.0	36.90

We reproduce again the direct measurements of r by Griffiths and Marshall :[4]

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[1] It may be remarked that this revision was prepared some years ago for some cryoscopic experimental investigations with benzene which I had intended to carry on.

[2] S. Young, *Jour. of Chem. Society*, Vol. LV., (1899) and Vol. LIX. (1891).

[3] The unit quantity of heat here adopted is about 4.189×10^6 ergs (A. Griffiths, *Phil. Mag.*, Vol. XL. (1858), p. 453).

[4] A. Griffiths and Marshall, *Phil. Mag.*, Vol. XLI. (1896).

HEAT OF VAPORIZATION *r* (ACCORDING TO GRIFFITHS AND MARSHALL).

t (Nitrogen Scale).	*r* Cal.	*t* (Nitrogen Scale).	*r* Cal.
19°.947C.	103.82	40°.045C.	100.71 ±0.057
30 .066	102.30 ±0.076	50 .014	99.14 ±0.034

The authors give the following linear formula as showing a very close approximation to the experimental values:

$$r = 107.05 - 0.1581 \times t.$$

When we plot these values (indicated by "experi." in the following table), it will be found that between 180° C. and 270° C. the representative points are regularly arranged, but quite irregularly between 80° C. and 180° C., showing, perhaps, that with Professor Young's sealed tube method "the results are more reliable at high than at low temperature." (Professor Young). Connecting them, however, by a mean curve I obtained the numbers (indicating by "curve") in the following table:

HEAT OF VAPORIZATION *r* (ACCORDING TO WRITER'S MEAN CURVE).

t	*r* (Curve.)	*r* (Experi.)	*t*	*r* (Curve.)	*r* (Experi.)	*t*	*r* (Curve.)	*r* (Experi.)
	Cal.	Cal.		Cal.	Cal.		Cal.	Cal.
10°	105.24	———	100°	91.05	91.60	190°	71.93	71.89
20	103.75	103.82	110	89.20	89.02	200	69.01	69.01
30	102.26	102.30	120	87.36	86.41	210	65.80	65.75
40	100.76	100.71	130	85.43	84.78	220	62.32	62.23
50	99.24	99.14	140	83.48	{82.06 {83.25	230	58.49	58.44
60	97.65	———	150	81.35	{80.17 {80.82	240	54.21	54.13
70	96.04	———	160	79.20	80.31	250	49.40	49.36
80	94.40	95.34	170	76.90	78.13	260	43.76	43.79
90	92.76	94.45	180	74.53	74.72	270	36.97	36.90

It will be seen from this table that my mean curve can be used for the purpose of interpolation and hence for some other purposes.

From these values of *r* we can calculate backwards the ratio of $\frac{v_i}{v_g}$ and then v_- taking as correct Professor Young's experimental values of v_i. I did this and found that the " mean values " of densities of the liquid and vapor obey in quite a satisfactory manner the empirical " law of straight diameter " (Cailletet and Mathias), not only from 80° C. to the critical temperature as remarked by Professor Young himself, but also from 80° C. down to the triple point. A more minute examination showed, however, the so-called diameter, in the case of *my* mean curve, to consist of two nearly straight parts meeting somewhere near the temperature 180° C., or more strictly, to be a curve whose curvature is so slight that it can be so described. This may indicate that *my* mean curve is not a good mean curve. But since we should ascribe much importance to the experimental values of Griffiths and Marshall, and surely to those that have been deduced from Professor Young's measurements between 180° C. and 270° C., I think that we must rather regard the empirical " law of straight diameter " as holding good in first approximation, but not in so reliable a degree as to draw from it any thermodynamic conclusions concerning benzene.

* * *

Another important source of information is the formula given by Regnault for the total heat (λ) :

$$\lambda = 109.0 + 0.24429 \times t - 0.0001315 \times t^2.$$

To make it comparable with the above results of calculation, and then to make some applications of it, it must first of all be subject to some thermometric and calorimetric reductions. Then by interpolations it would become possible to calculate, if we make use of the above result, the specific heat of saturated vapor, and to obtain perhaps quite a satisfactory formula for the specific heat of liquid benzene at higher temperatures ; or to calculate, if we take the formula in conjunction with some reliable measurements on the specific heat of liquid benzene, the heat of vaporization at various temperatures, and then see how far do the results coincide with the calculations of this kind given by Wüllner.[1] As, however, I do not possess Regnault's original memoirs, I was unable to examine these points.

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[1] A. Wüllner, Experimental Physik, Bd. III. (4to Auf.), 5, 726.

Let us now turn to the vapor pressures of benzene below its triple point (+ 5°.3 C.).

The measurements of these by Fischer,[1] Ramsay and Young,[2] and Ferche[3] were plotted on section paper and mean curves drawn to pass well through the representative points. This was done twice quite independently, and when the results obtained were compared, it was found that for liquid benzene they differed very little in the cases of Fischer's and Ferche's measurements, generally by 0.01–0.03 mm. of mercury; but with Professor Ramsay and Young's they differed sometimes by 1.5 mm., perhaps owing to a comparatively small number of points available for drawing the mean curve between the triple point and −1° C.

VAPOR PRESSURE OF LIQUID BENZENE.

t	Fischer.	R. & Y.	Ferche.	Mean Curve.	t	Fischer.	R. & Y.	Ferche.	Mean Curve.
	m.m.	*m.m.*	*m.m.*	*m.m.*		*m.m.*	*m.m.*	*m.m.*	*m.m.*
− °1	24.87	25.04	25.03	24.98	+2°.5	30.28	30.25	30.37	30.30
−0 .6	25.60	25.73	25.68	25.67	3	31.11	31.11	31.23	31.55
0	26.34	26.42	26.41	26.39	3	31.99	32.00	32.10	32.03
+0 .5	27.10	27.14	27.16	27.13	3 .5	32.87	32.94	33.01	32.94
1	27.88	27.88	27.91	27.89	4	33.78	33.89	33.94	33.87
1 .5	28.66	28.65	28.69	28.67	4 .5	34.70	34.88	34.87	34.82
2	29.46	29.43	29.52	29.47	5				

Proceeding in a similar way with solid benzene, I obtained the following values from the different authors' sets of measurements. Things are less favorable in the present case than in the preceding, in fact, not only did my own mean curves, several of which I drew for one set, differ much among themselves in the cases of Professor Ramsey and Young's and Fischer's measurements, but also the different sets compared with one another showed greater discrepancies than in the preceding case. It will be seen that my mean curve runs on the whole quite close to Ferche's, that of Fischer lying nearly as much above the latter as Professor Ramsey and Young's lies below it.

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[1] W. Fischer, *Wiede. Ann.*, Bd. 28 (1886).

[2] W. Ramsay and S. Young, *Phil. Mag.*, Vol. 23 (1887).

[3] J. Ferche, *Wiede. Ann.*, Bd. 44 (1891).

VAPOR PRESSURE OF SOLID BENZINE.

t	Fischer.	R. & Y.	Ferche.	Mean Curve.	*t*	Fischer.	R. & Y.	Ferche.	Mean Curve.
	m.m.	m.m.	m.m.	m.m.		m.m.	m.m.	m.m.	m.m.
−1°C.	23.21	21.97	22.52	22.57	+2°.5C	29.30	28.97	29.14	29.14
−0.5	24.03	22.94	23.40	23.46	3	30.25	30.05	30.19	30.16
0	24.88	23.90	24.30	24.36	3.5	31.25	31.16	31.27	31.23
+0.5	25.72	24.89	25.23	25.28	4	32.28	32.32	32.36	32.32
1	26.58	25.88	26.18	26.21	4.5	33.35	33.52	33.50	33.46
1.5	27.46	26.89	27.13	27.16	5	34.50	34.75	34.68	34.64
2	28.36	27.91	28.12	28.13					

Following the line of reasoning used by Guldberg[1] we can obtain a thermodynamic relation between p_i and p_s. By the fundamental principles of reversible thermodynamics considered as applicable to a substance in any of the two coexisting phases of it, we have the well known formulæ: in the case of liquid and vapor phases.

$$r_{gi} = T(v_g - v_i)\cdot\frac{dp_i}{dT},$$

and

$$h_{gi} = T.\,d\frac{\left(\frac{r_{gi}}{T}\right)}{dT} + c_i;$$

in the case of vapor and solid phases,

$$r_{gs} = T(v_g - v_s)\cdot\frac{dp_s}{dT},$$

and

$$h_{gs} = T.\,d\frac{\left(\frac{r_{gs}}{T}\right)}{dT} + c_s,$$

where $h_{gl}\,(h_{gs})$ and $c_l\,(c_s)$ develop respectively the specific heats along vapor- and liquid- (solid-) lines. Assuming the vapor over the liquid and solid states to be the same, we may put

$$h_{gi} = h_{gs},$$

so that

$$c_i - c_s + T\frac{d}{dT}\left(\frac{r_{gi} - r_{gs}}{T}\right) = 0.$$

By integration we obtain

$$\int\frac{c_i - c_s}{T}\,dT = \frac{r_{gi} - r_{gs}}{T} + k\ \text{(const.)}.$$

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[1] Guldberg, C. R. (1870).

Assuming $c_i - c_s$ as constant we obtain

$$(c_i - c_s) \log T = \frac{r_{gs} - r_{gi}}{T} + k,$$

which gives $$(c_i - c_s) \log \frac{T_0}{T} = \frac{F_0}{T_0} - \frac{1}{T} r_{gs} - r_{gi}),$$

where $F_0 (= r_{gs} - r_{gi}$ exactly at the triple point) denotes the heat of fusion at that temperature. When we neglect v_i as well as v_s in comparison with v_g and make the further assumption that Boyle-Gay-Lussac's law together with Avogadro's "hypothesis" holds good for the vapor of benzene between the triple point and near 0°C., we obtain

$$\frac{1.971}{78} \cdot T \cdot \frac{d}{dT}\left(\log \frac{p_s}{p_i}\right) = \frac{F_0}{T_0} - (c_i - c_s) \log \frac{T_0}{T} .$$

Hence, by integration and some transformation,

$$\log \frac{p_s}{p_i} = \frac{78}{1{,}971} \cdot \log \frac{T.}{T_0} \cdot \left(\frac{F_0}{T_0} + \frac{c_i - c_s}{2} \log \frac{T}{T_n} \right),$$

which becomes, when we make some neglections,

$$\log \frac{p_s}{p_i} = \frac{78}{1{,}971} \cdot \frac{\tau \,.\, F_0}{T_0^2} ,$$

exactly as given by Guldberg.

In the last but one formula I have put

$$\begin{aligned} T_0 &= 278.3, \\ F_0 &= 30.00, \\ \left.\begin{aligned} c_i &= 0.3350, \\ c_s &= 0.2032 \end{aligned}\right\} & \ \text{(Ferche.)} \end{aligned}$$ [1]

and obtained the numbers given in the first three columns of the following table:

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[1] Here we may put c_i and c_s equal to the specific heats at constant pressure of liquid and solid benzene. M. Fischer puts the specific heat of solid benzene equal to 0.319, while M. Ponsot (C. R., 1894) adopts the value 0.017 of the difference of the two specific heats without giving any reason for it. Though of no serious effect in the present case, yet such a difference considered in itself requires an experimental examination.

VAPOR PRESSURES OF SOLID AND LIQUID BENZENE.

t	p_s	$\frac{p_l}{p_s}$	p_s (Fischer.) minus.	p_s (R. & Y.) minus.	p_s (Ferche) minus.	p_s (Mean.) minus.
	Calculated.	Calculated.	p_s (Calc.)	p_s (Calc.)	p_s (Calc).	p_s (Calc.)
	m.m.		m.m.	m.m.	m.m.	m.m.
−1° C.	22.69	1.1011	+0.52	−0.72	−0.17	−0.12
−0 .5	23.49	1.0927	+0.54	−0.55	−0.09	−0.03
0	24.34	1.0844	+0.53	−0.44	−0.04	+0.02
+0 .5	25.21	1.0761	+0.51	−0.32	+0.02	+0.07
1	26.11	1.0680	+0.47	−0.23	+0.07	+0.10
1 .5	27.05	1.0599	+0.41	−0.16	+0.08	+0.11
2	28.01	1.0518	+0.35	−0.10	+0.11	+0.12
2 .5	29.03	1.0438	+0.27	−0.06	+0.11	+0.11
3	30.07	1.0359	+0.18	−0.02	+0.12	+0.09
3 .5	31.16	1.0280	+0.09	0	+0.11	+0.07
4	32.29	1.0201	−0.01	+0.03	+0.07	+0.03
4 .5	33.46	1.0123	−0.01	+0.06	+0.04	0
5	34.66	1.0046	−0.16	+0.09	+0.02	−0.02

The numbers in the last four colums may be taken as indicating teat the several assumptions we have made in deducing the formula employed as well as the several experimental data we have used in calculating the vapor pressures, are both of them not very wrong. Hence the formula with the data may be used as one of first approximation.[1]

TOKYO, JAPAN.

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[1] By the way, let me here add the following. I obtained the following values of the constants of Biot's form of interpolation formula; choosing the mean values of pressure at − 0°.5, + 2°, and + 4°.5 C. for liquid benzene:

$$\log p_l = a + ba^t,$$

$$a = \overline{7}.\ 8749340,$$

$$\log b = 0.\ 8770540,$$

$$\log a = 0.\ 0013878.$$

Similarly for solid benzenes:

$$\log p_s = a' + b'\rho^t$$

$$a' = 3.\ 153721,$$

$$\log b' = 0.\ 2512469,$$

$$\log \beta = \overline{1}.\ 9921452.$$

AN EFFICIENT BOYLE'S–LAW APPARATUS.

W. J. HUMPHREYS.

NOTHING is more discouraging to teacher or student than a piece of apparatus incapable of giving consistent and approximately accurate results. For this reason the ordinary U shaped tube, used for demonstrating the truth of Boyle's law, is one of the least satisfactory things the junior laboratory student has to work with. No matter how careful his measurements, irregularities in the bore of the tube, even where all other sources of appreciable error are avoided, will lead to irregular and unsatisfactory determinations.

The importance of apparatus capable of yielding good results, however elementary the experiments, is my excuse for offering the following description of a Boyle's law device which I have recently had constructed, and which, according to tests, readily gives values that do not differ from the correct ones by more than one part in a thousand.

Referring to the figure : *V* is a tube about twelve centimeters long and one centimeter in diameter having a two-way stop-cock at eachend. *D* is a long tube of about the same diameter as *V* to avoid any appreciable difference in capillary action. *D* may be sealed to *V* or connected to it in any way desired—the figure shows the method I have adopted. In the event of any leakage the short rubber tube *R* may be made tight either by wrapping or by putting it on the glass tubes with shellac varnish. However I have not found either of these precautions necessary.

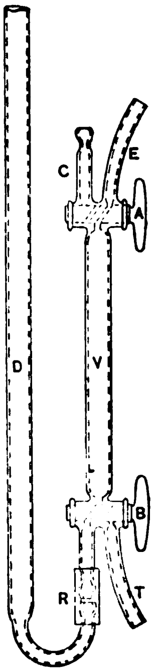

Fig. 1.

The stop-cocks, of course, must be air tight. An excellent lubricant for them is paraffine, with just enough vaseline added to keep it from being brittle.

The operation is as follows: Close stop-cock B and fill D to any desired height with mercury, then open B and allow a small amount of mercury to run into V, then turn B till V and T are in communication. This lets the mercury out of V but leaves the tube through B that connects V and D entirely filled. Now close A and the volume of air in V is perfectly definite and at atmospheric pressure. Next turn B till V and D are again in communication. The mercury will now rise slowly in V and be free from all air bubbles. When the mercury has come to rest close B and read the difference in mercury levels in D and V, either with a cathetometer or with a meter bar placed between D and V. Now open A and B and run the mercury out T and weigh it. Obtain similar results for as many different heights as desired.

Lastly leave V open to E say and run mercury up V and into A, close B, open V to C, then run the mercury out T and weigh. This gives the weight of the mercury necessary to fill V absolutely.

Let $W =$ weight of mercury required to fill V,
" $w =$ " " " run in when A is closed,
" $p =$ difference in heights of mercury in D and V,
" $B =$ height of barometer at time of experiment,
" $V =$ volume of tube V,
" $v =$ " " compressed gas in V.

Then

$$\frac{W}{W-w} = \frac{V}{v} = \frac{B+p}{B},$$

and

$$B = \frac{p(W-w)}{w}.$$

Of course B may be obtained directly from a barometer, or it can be determined to within a fraction of a millimeter by the method just shown, its value substituted in the equations, and Boyle's law tested.

This is one method, and occasion to use it might arise, of obtaining a fairly satisfactory barometric reading without the use of a standard barometer. I have described the apparatus as used for pressures above one atmosphere. It can be used, however, with equal ease and accuracy for pressures less than atmospheric, but the operations are so obvious and simple that a description of them would be superfluous, except possibly the statement that it is necessary to determine the weight of the mercury required to fill the tube in B that connects V with T.

If the student has time he may simply assume that, at constant temperature, the volume of a gas is some fixed function of the pressure. His values will show him what this function is, determine the height of the barometer, and in every way give him a most satisfactory demonstration of the truth of Boyle's law.

It may be not out of place to say that in filling D it is advisable to introduce the mercury through a small funnel or thistle tube, with its stem drawn out to a fine opening. This prevents fouling the walls of the tube, the dirt being collected on the sides of the funnel.

ROUSS PHYSICAL LABORATORY, UNIVERSITY OF VIRGINIA, Dec. 7, 1899.

NEW BOOKS.

Strahlung und Temperatur der Sonne. By Dr. J. Scheiner. Pp. i–v, 1–99. W. Engelmann, Leipzig, 1899.

This monograph on solar temperatures shows the importance which is now attached to Stefan's law, particularly in Germany. Some time has elapsed since Stefan noticed that the observations of Dulong and Petit, de la Provostage and Desains, Draper, Ericsson and others could be satisfactorily interpreted by assuming the quantity of heat radiated by a body to be proportional to the fourth power of its absolute temperature. Boltzmann thereafter deduced the same law from the electromagnetic theory of light by aid of the second law of thermodynamics. Boltzmann himself insisted on the provisional character of his proof; but the law has recently received independent experimental confirmation at the hands of Lummer and Pringsheim (1560° C.) and others, operating with absolutely black bodies, *i. e.*, with hollow equally heated radiators emitting heat from within. For such bodies only the law appears to be valid.

Dr. Scheiner, therefore, assumes the truth of Stefan's law to this extent, and defines the sun's effective temperature, as the temperature which it would have if it radiated like a black body. From the known value of the solar constant, s (heat received per sq. cm., per sec.) and the known heat emission, h, of a black body for a definite temperature interval (100°, Kurlbaum and others), it is thus possible to arrive at trustworthy values for the temperature of the sun t. If φ be the apparent solar diameter, Stefan's law leads approximately to

$$t = 273 \sqrt[4]{\frac{2.843}{\sin^2 \frac{\varphi}{2}} \frac{s}{h}},$$

and Scheiner in this way finds $t = 7010°$. Treating all earlier observations by the same method he computes the following list of independent values:

Pouillet,	$t =$ 5600°	Langley,	$t =$ 6000°
Secchi,	5400°	Wilson & Gray,	6200°
Violle,	6200°	etc.	
Soret,	5500°		

In view of the enormous discrepancies in the values deduced for the sun's temperature by these observers themselves (data between 1500° and

10,000,000°), the author concludes that the consistent values of the present method must commend the problem to the serious consideration of astronomers. The reviewer fails to see however, why similar data similarly interpreted, do not necessarily lead to comparable results.

Corrected for solar absorption, Scheiner's values for the effective temperature of the sun's photosphere is 7760° C.

Among many indirect methods for estimating the sun's temperature, the one based on the position (λ) of the energy maximum in the solar spectrum deserves mention here. Langley's observations (maximum at $\lambda = 0{\cdot}6\mu$) interpreted by Wien put the sun's effective temperature at 4630°, in reasonable agreement with the above data.

The monograph contains a full digest of correlative data in solar physics. The absorption of the earth's atmosphere is treated in detail. A complete account is given of solar radiation, considered both photometrically and thermally, including a summary of the laws of radiation hitherto proposed. Much space is devoted to the distribution of light over the sun's disc, to the absorption of the solar atmosphere, to secular and periodic changes of the sun's temperature, to the electrodynamic radiation of the sun, etc. Any student interested in the profound cosmical problems treated will profit by the varied information given in this useful little book.

Carl Barus.

Geschichte der Physikalischen Experimentierkunst. By E. Gerland und F. Traumüller. Pp. 442. (14 M.; bound, 17 M.). W. Engelmann, Leipzig, 1899.

The general history of physics embraces not only physical theories, but also the discovery of physical facts upon which the theories rest and the art of experimentation, by which the facts were ascertained. The above book limits itself to the last of these functions. The authors trace the history of the art of experimentation from early times down to the beginning of the present century. The 425 figures show some ingenious devices which are not usually explained in historical works. Here we find illustrated the ancient Egyptian process of glass-blowing, the Archimedian endless screw, several of Hero's curious devices, the famous "tower of the winds" in Athens, which testifies to an early interest in meteorology, the Roman steelyards, the hydrometer of Synesius, Al Khâzinî's "balance of wisdom," Gilbert's "instrumentum declinationis," Galileo's pendulum clock, Guericke's quaint illustrations of experiments on atmospheric pressure, Agricola's and Papin's mine ventilators, and many kinds of apparatus devised during more recent times. The work of the nineteenth century remains untouched, except the researches on electricity and magnetism during the first forty years.

The experiments of the *Accademia del Cimento* of Florence are described with greater fullness than we have seen in other histories. Extremely interesting are the series of experiments on the compressibility of water, culminating in the well-known test with the hollow spheres of silver which were filled with water, closed tight, and then disfigured by hammering until the water forced its way through the pores (or cracks ?) of the metal. That the Academicians did not feel justified by their experiments to draw the conclusion that water is incompressible, speaks highly for the caution which they exercised in their researches. Their report on the subject ends as follows: "It is certain that, as compared with air, water withstands an infinitely greater pressure (if we may so express ourselves), which confirms what we said at the beginning. While experimental research does not always lead us to the most remote truths which we seek, it gives us, nevertheless, in each favorable or unfavorable case, some light, be it ever so little."

The struggle for supremacy between alcohol and mercury as thermometric substances is told in an attractive manner, but the authors fail to record the interesting fact brought to light by M. Maze (*Comptes Rendus*, Vol. 120, 1895, p. 732), that in 1559 more than half a century before Fahrenheit, a mercury thermometer was constructed by the astronomer Boullian.

The learned German authors point to Seebeck as one who approached closely to the discovery of Ohm's Law three years before Ohm made his experiments in 1826, but they have overlooked the fact established by Maxwell (*Electrical Researches of the Hon. Henry Cavendish*, 1879, p. LIX.), that Cavendish as early as 1781 completed a series of experiments which amount to an anticipation of Ohm's Law.

The authors describe the electrical researches of Faraday, but do not mention Joseph Henry, who, while a teacher at the Albany Academy, made the discovery of magneto-electricity about the same time as Faraday, and who antedated Faraday in the discovery of self-induction. The authors also speak of Morse without mentioning Henry, although it is quite firmly established that Morse's assistant, Dr. Gale, applied the principle discovered by Henry to render Morse's machine effective at a distance. (*Smithsonian Report* 1857, pp. 99–106.)

But these are minor blemishes. The work as a whole is accurate and up-to-date in the information which it imparts. It contains much material otherwise difficult of access.

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Volume X. *March, 1900.* *Number 3.*

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THE
PHYSICAL REVIEW.

ON THE METABOLISM OF MATTER IN THE LIVING BODY.

By Edward B. Rosa.

A GREAT many so-called metabolism experiments have been made upon the human body and upon animals in which a balance of nitrogen or of nitrogen and carbon was obtained. That is, the quantity of nitrogen or of nitrogen and carbon in the food was determined by analyzing samples of the food, and the quantity of these elements in the excreta and the products of respiration was also determined. If the nitrogen taken into the body during the course of an experiment exceeds that given off from the body in the same period, then there is a net gain of nitrogen, and this is taken to indicate an increase in the total amount of protein of the body. Conversely, if the outgo of nitrogen exceeds the receipts there is a net loss, and this indicates a loss of protein. As protein contains 16 per cent. of nitrogen, the quantity of protein gained or lost is found by multiplying the gain or loss of nitrogen by 6.25. But protein also contains 53 per cent. of carbon. Hence, before striking a balance of the carbon, we must insert on one side or the other of the account, the quantity of carbon in the protein which has been gained or lost. The net gain or loss of carbon is then taken as a measure of the amount of fat which has been gained or lost from the body during the experiment. As fat contains 76.5 per cent. of carbon, the quantity of fat gained or lost is found by dividing the gain or loss of carbon by the factor .765.

These experiments in which both a nitrogen and carbon balance are determined require a determination, as has been said, not only of the nitrogen and carbon of the food and excreta, but also of the quantity of carbon dioxid given off from the lungs and skin; that is, of the products of respiration. They are hence called respiration experiments. Sometimes the subject of the experiment has worn a mask in such a manner as to respire air drawn from a particular source, the composition of which is determined, while the air expelled from the lungs is carried away to a reservoir, and its contents of oxygen and carbon dioxid subsequently determined. This gives the quantity of oxygen absorbed and of carbon dioxid exhaled. But such an experiment neglects the respiration of the skin, and is unsatisfactory on other accounts. The better method is to confine the subject in a chamber and to analyze samples of the air supplied to the chamber, and also other samples drawn from the current of air flowing away from the chamber.

In the experiment reported by Atwater and Rosa in the September and October numbers of the PHYSICAL REVIEW, as well as in subsequent experiments with the same apparatus, a nitrogen and carbon balance was obtained and also a balance of energy. For the latter, the potential energy of the food eaten by the subject was determined by burning samples in a calorimeter; the potential energy of the excreta was determined by burning samples of the dried residue in a similar manner; the heat given off from the body during the experiment was also measured, the respiration chamber being at the same time a calorimeter, hence called a respiration calorimeter. A balance of energy can then be reached by taking account of the quantity of protein and fat gained or lost. Such a balance of energy, to within about one or two per cent. has been obtained in a considerable number of experiments at Middletown, and the work is still being carried on.

A balance of hydrogen is more difficult to obtain than are the nitrogen and carbon balances. This involves, of course, the determination of the organic hydrogen of food and excreta, and also the quantity of water in the same and the quantity of water exhaled from the skin and lungs. The water vapor contained in the current of air entering the calorimetric chamber, and also that leaving through the outgoing current of air are determined by absorption in sulphuric acid tubes at the same time that the carbon dioxid is absorbed by soda lime. But water being to some extent condensed upon the walls of the chamber, and absorbed by the clothing of the subject and the furnishings of the chamber, the exact determination of the water becomes a more difficult matter than that of the carbon dioxid, and a hydrogen balance has been seldom attempted.

It is impossible for one who has not been engaged in it to appreciate the difficulty and complexity of this kind of experimental work, or the enormous amount of labor and computation necessary to obtain the final results of a four days' respiration experiment. In the Middletown experiments analyses of samples of food were made in duplicate, and all determinations of carbon dioxid and water in the air current were in duplicate. But in spite of the greatest care residual errors are not inappreciable. And in weighing the food and drink, selecting samples for analysis, and in collecting and weighing the water condensed inside the chamber and outside in the freezers, errors of appreciable magnitude are bound to occur. Hence when one strikes a balance of nitrogen and carbon and computes the number of grams of protein and fat which have been gained or lost, the algebraic sum of a large number of errors appears in the result. Moreover, in an energy experiment this error is carried over into heat account in a magnified degree. Suppose, for example, that in the determination of the carbon of the food there is an error of one-half of one per cent.; the quantity of carbon in the daily ration being 261.5 g., this amounts to an error of 1.3 g., or 1.7 g. of fat. The heat value of a gram of fat being 9.4 large calories, 1.7 g. is equivalent to 16.0 calories, which is 0.7 per cent. of the total heat per day. An error in the nitrogen, however, produces a comparatively small error in the computed heat; for if the protein gained be reckoned too large, the fat gained will be too small in a corresponding degree, and the difference of heat value produced is relatively small.

In order to locate the sources of error and to reduce them in magnitude to a minimum, it is desirable to make use of every possible check upon the analyses and computations. Such a check may be found in the change of weight of the subject of the experiment, provided the oxygen taken up from the air by respiration can be determined. Direct determinations of oxygen have been made in short experiments, by analyzing the air in the chamber at the beginning and at the end, and admitting to the chamber during the experiment a known quantity of oxygen. But for long experiments this is impracticable, as the air of the chamber must be frequently renewed. On the other hand, it is impossible to analyze the air by sample, as is done for CO_2 and H_2O, and determine the percentage of oxygen in the ingoing and outcoming air currents with sufficient accuracy. For if 50 liters of air be admitted to the chamber per minute, the quantity of oxygen in 24 hours will be about 20,000 g. and an error of one-tenth of one per cent. in the determination would amount to 20 grams, which would be an error of three per cent. in the amount of oxygen absorbed by the lungs.

On the other hand, an oxygen balance may be obtained in a manner similar to that followed for N, C and H. If the quantity of these latter elements and the ash of the food and excreta are accurately determined, then the amount of oxygen becomes known. The oxygen of the water of income and outgo is accurately known when the quantity of water is determined, and hence one can readily calculate the total amount of oxygen taken into the body and the total given out. The difference will represent that derived from the atmosphere by respiration. If it be objected that this determination of oxygen by difference is not sufficiently exact it may be replied, first, that if the oxygen is not exact then the other elements, N, C and H, are not exact; and, second, that the dry residue of food and excreta contains over 30 per cent. of oxygen, a greater proportion than that of any other constituent except carbon, and hence a determination by difference is far more reliable than where that difference is a small quantity, as, for example, in the case of the protein and fat gained or lost. But the latter quantities are regularly calculated by difference, with no check upon the results to show them to be correct. It is now proposed in a similar manner to calculate the hydrogen and oxygen by difference, and then to determine the oxygen taken up from the air in the manner of Pettenkofer and Voit,[1] from the gain or loss of weight of the

[1] The accuracy of weighing of these distinguished investigators was evidently not sufficient for the purpose

subject, and to compare the two results thus derived by two independent methods. Or, having computed in the manner already indicated the amount of oxygen absorbed from the atmosphere, we are to calculate the gain or loss of weight of the person or animal which is the subject of the experiment. And if this weight is ascertained by an accurate balance at the beginning and end of the experiment, we have a means of checking by the difference the accuracy of the experiment. But if the subject is weighed four times each day, that is at the beginning of each six-hour period, and the observed weight tallies each time closely with the computed weight, we have a still better verification of the work. The probability is that considerable discrepancies would appear between the observed and computed weights, which would point out the more important sources of error, and thus serve as a means of increasing the accuracy of the work, besides sometimes detecting accidental errors of importance which would not otherwise be noticed.

It was such considerations as these that led to the purchase several years ago of an accurate scale, capable of weighing up to 100 kilos, which was put inside the calorimeter and the subject weighed himself several times during each experiment. However, this check was not applied, and the weighings, though made and recorded were never made use of. Having now retired completely from the investigation, I wish to present here the method which I devised about five years ago and to work it out for the experiment that was published in the recent numbers of the PHYSICAL REVIEW. The weighings were not as accurately nor as frequently made as they might have been; indeed, for this purpose they would better be made by the observers outside, and not by the subject within. I will indicate later how this can readily be done.

Table I, gives the balance of N,C,H,O and ash for the four days taken together. Under each element are two columns headed "In" and "Out," containing respectively the amounts of that element in the income of the body from all sources and the outgo of all kinds. These columns should balance, after the manner of the debit and credit columns of a ledger account, when one has inserted the balancing item. In the case of nitrogen this is the amount of nitrogen acquired from the protein of the tissues which was broken down

TABLE I.

Balance of Nitrogen, Carbon, Hydrogen and Oxygen.

Materials of Income and Outgo.		Total.	Nitrogen.		Carbon.		Hydrogen.		Water.		Oxygen.		Ash.	
			In.	Out.	In.	Out.	In.	Out.	In.	Out.	In.	Out.	In.	Out.
		Grams.												
Food, Water.	Income.	3684.0					412.0		3684.0		3272.0			
" Solids.		2208.0	76.4		1046.2		150.4				845.8		89.2	
Water of Drink.		5382.0					602.0		5382.0		4780.0			
Oxygen from Air.		2611.6									2611.6			
Feces, Water.	Outgo.	308.8						34.5		308.8		274.3		
" Solids.		115.2		5.0		53.4		7.3				30.5		19.0
Urine, Water.		6421.0						718.2		6421.0		5702.8		
" Solids.		281.4		73.7		50.3		13.4				68.0		76.0
Water of Respiration.		3765.0						421.1		3765.0		3343.9		
Carb. Dioxide of Res.		3279.8				894.5						2385.3		
From Comb. of Body Protein.		14.4	2.3		7.6		1.0				3.5			
" Storing of Body Fat.		72.7				55.6		8.7				8.4		
" Dimin. of Body Water.		338.1					37.8		338.1		300.3			
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.		150.4+1.0 −(7.3+13.4 +8.7)=122.0							1090.7				5.8	
			78.7	78.7	1053.8	1053.8	1203.2	1203.2	10494.8	10494.8	11813.2	11813.2	95.0	95.0

over and above what has been formed; for carbon, the amount contained in the fat stored away during the experiment; for hydrogen, the amount derived from the decrease of the water contained in the body; for oxygen, the amount derived from the atmosphere; and for ash, the loss of mineral matter of the body. These balancing items are printed in bold faced type.

The daily ration weighed 1,473 g., of which 921 g. was water and 552 g. dry solids. Thus in four days the water amounted to 3,684 g. and the dry solids to 2,208 g. These quantities are entered in the table in the column of totals. Taking the atomic weights of hydrogen and oxygen as 1 and 15.88, water contains 11.185% of hydrogen and 88.815% of oxygen. The water of the food, therefore, contains 412 g. of hydrogen and 3,272 g. of oxygen, and these items are entered in the proper columns under hydrogen and oxygen respectively. Analysis of samples showed that the dry residue contained 76.4 g. of nitrogen, 1,046.2 g. of carbon, 150.4 g. of hydrogen, and 89.2 g. of ash. The remainder, 845.8 g., is oxygen. All these items are entered in the table in their appropriate places. The 5,382 g. of water drunk (more than half of this was in coffee), in addition to that contained in the food, consisted of 602 g. of hydrogen and 4,780 g. of oxygen. The amount of water is also entered in the column of water. The fourth item of income, oxygen received from the air, is entered last of all, being a balancing item.

We get an approximate check upon the oxygen of the food in the following manner. The dry residue contained:

Protein, ...	478.4 g.,	of which	24.0%	is oxygen	= 114.8 g.
Fat, ...	276.0	" "	11.5%	"	= 31.6
Carbohydrates, ...	1364.4	" "	50.0%	"	= 682.2
Ash, ...	89.2	"			0.
Total	— 2208.0 g.			Total oxygen	— 826.6 g.

As these percentage compositions are not exact we cannot regard this as more than a rough check upon the direct analyses, although the true value may perhaps lie between the two.

The next four lines in the table give the amounts of the various elements in the solid and liquid excreta, the total weights of which are inserted in the column of totals, and the quantity of each element in the "out" column of that element. The water and carbon dioxid of respiration in like manner go into the column of output, the latter being exactly $\frac{8}{11}$ carbon and $\frac{8}{11}$ oxygen; that is, the atomic weight of carbon is 11.91 if oxygen is 15.88, or 12 if oxygen be taken at 16.

We now come to the balancing items:

First, the output of nitrogen is 2.3 g. more than the income. We therefore insert 2.3 as the balance, and this multiplied by 6.25 gives 14.4 g. as the weight of protein lost. This is entered in the column of totals and 7.6 g. of carbon, 1 g. of hydrogen and 3.5 g. of oxygen also contained in the protein are charged into the debit side of those accounts.

Second, the receipts of carbon exceed the output by 55.6 g. Hence we insert this as the balancing item and put 72.7 g. in the column of totals as the weight of the fat in which this carbon is stored up; 8.7 g. of hydrogen and 8.4 g. of oxygen are also contained in this fat, and these items are accordingly entered in their proper places.

Third, the output of hydrogen exceeds the receipts by 37.8 g. This is explained by the fact that the amount of water in the body must have decreased during the experiment by 338.1 g., for no other source of hydrogen remains to be considered. This is indeed the only way of ascertaining the change in the content of water in the body. The accuracy of this determination of course depends upon the accuracy of the several items occurring in the hydrogen columns. There is reason to believe they are less accurate than those of the nitrogen and carbon accounts, but the result of this computation is to give a check which tests the hydrogen account more than any other in the table.

Fourth, the item of 338.1 g. of water lost must be entered in the debit column of the water account, and 300.3 g. in the oxygen account. It represents the amount derived from the body's capital stock at the beginning of the experiment. To balance the water account we must take note of the fact that the organic hydrogen of the food when it is oxidized forms water, which must be charged into the debit side of the water account; or rather, the excess of hydrogen contained in the food (150.4) and that contained in the protein used up (1.0), over that contained in the feces (7.3), urine (13.4) and that stored up in the fat accumulated (8.7). Thus: $150.4 + 1 - (7.3 + 13.4 + 8.7) = 122.0$ g. of hydrogen, which has been oxidized and formed 1090.7 g. of water. Entering this item on the debit side of the water account, the latter balances exactly.

Fifth, the oxygen account is now complete except the balancing item of 2611.6 g., which represents the quantity of oxygen derived from the atmosphere. This assumes that whatever quantity of oxygen is stored up as such or as oxyhaemoglobin in the blood and in the tissues does not vary by an appreciable amount; or, at least, that it was sensibly the same at 7 o'clock on the morning that the experiment began that it was at 7 o'clock on the last morning. This shows an absorption of 27.2 g. per hour or .45 g. per minute.

The balance of oxygen may be put in another form by omitting from both sides of the account the oxygen from the water which is received into the body. It then appears as follows:

BALANCE OF OXYGEN FOR FOUR DAYS.

	Receipts.	**Output.**
From the food	845.8	
From 14.4 g. protein of the body	3.5	
Excreted in feces (solids)		30.5
" urine "		68.0
" carbon dioxid		2385.3
" water from organic hydrogen		968.7
Stored away in fat		8.4
Balance from atmosphere	2611.6	
	3460.9	3460.9

Sixth, the column of ash does not balance as closely as it ought. There could not be a loss of 5.8 g. of mineral matter, and hence this difference must be mainly due to error in some one or more of the items given. Indeed, the record does not show positively whether all three items were actually determined. A change in any of them would alter the oxygen item by the same amount, but this would affect the final result only slightly, and for the present purpose it makes no difference.

We are now in position to calculate the loss of weight of the person under investigation, during the four days he was confined in the respiration chamber. We must first note the fact that the feces passed during these four days weighed 498.4 g., being 74.4 g. more than those which belonged to that period, the latter being distinguished by the use of charcoal.

BALANCE OF MATTER.

Income.	Grams.	Outgo.	Grams.
Food	5892.0	Feces	498.4
Drink	5382.0	Urine	6702.4
Oxygen from air	2611.6	Water of respiration	3765.0
Balance = Loss of weight	360.0	Carbon dioxid of respiration	3279.8
	14245.6		14245.6

This loss of 360 grams in weight is made up as follows :

Loss of water from body, see Table I., =	338.1 g.
" " feces " "	74.4
" " protein " " " " " =	14.4
" " ash " " " " "	5.8
	432.7
Gain of fat, " " "	72.7
Net loss of weight, as above,	360.0

Now the weight of the man at the beginning was 68,420 g. and at the end was 68,000[1] g., making a loss of 420 g., or 60 g. more than the calculated loss. This is of course not a very close agreement. The discrepancy may be partly accounted for by the fact the weighings of the subject by himself were not as exact as if made by some one else, and were not made at the exact time of beginning and end. The computed loss of weight is of course in error by an appreciable amount, but for want of more reliable weighings we cannot say definitely what the error is.

Tables II. and III. give the results of a similar computation for each day separately. The food was the same each day, although there was a slight variation of the water of the food and on the first day the water of the drink was about 200 g. more than on the subsequent days. The feces, although passed in only two portions, were assumed to be separated in equal quantity each day. There was only a slight variation from day to day in the evolution of

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[1] The record of this weight is 67,200, but from a previous weight it is evident that this is an error and should be 68,000.

TABLE II.

Balance of Nitrogen, Carbon and Hydrogen.—By Days.

Materials of Metabolism.		**Nitrogen.**							
		1st Day.		**2d Day.**		**3d Day.**		**4th Day.**	
Food, Water.	Income.								
" Solids.		19.1		19.1		19.1		19.1	
Water of Drink.									
Oxygen from Air.									
Feces, Water.	Outgo.								
" Solids			1.2		1.3		1.2		1.3
Urine, Water.									
" Solids.			18.8		18.7		18.3		17.90
Water of Respiration.									
Carb. Dioxide of Res.									
From Comb. of Body Protein.		0.9		0.9		0.4		0.1	
" Storing of Body Fat.									
" Dimin. of Body Water.									
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		20.0	20.0	20.	20.	19.5	19.5	19.2	19.2
Protein Consumed:		5.65		5.65		2.5		0.6	
						Total = 14.4			

Materials of Metabolism.		**Carbon.**							
		1st Day.		**2d Day.**		**3d Day.**		**4th Day.**	
Food, Water.	Income.								
" Solids.		261.5		261.5		261.6		261.6	
Water of Drink.									
Oxygen from Air.									
Feces, Water.	Outgo.								
" Solids.			13.3		13.3		13.4		13.4
Urine, Water.									
" Solids.			12.8		12.8		12.5		12.2
Water of Respiration.									
Carb. Dioxide of Res.			226.7		221.8		220.7		225.3
From Comb. of Body Protein.		3.0		3.0		1.3		0.3	
" Storing of Body Fat.			11.7		16.6		16.3		11.0
" Dimin. of Body Water.									
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		264.5	264.5	264.5	264.5	262.9	262.9	261.9	261.9
Fat Stored:		1.52		21.7		21.4		14.4	
						Total = 72.7			

TABLE II.—*Continued.*

Materials of Metabolism.		Hydrogen.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.	103.0		102.8		103.0		103.2	
" Solids.	Income.	37.6		37.6		37.6		37.6	
Water of Drink.	Income.	167.0		144.3		145.4		145.3	
Oxygen from Air.	Income.								
Feces, Water.	Outgo.		8.6		8.6		8.7		8.6
" Solids.	Outgo.		1.9		1.8		1.8		1.8
Urine, Water.	Outgo.		199.5		213.1		161.3		144.3
" Solids.	Outgo.		3.4		3.4		3.3		3.3
Water of Respiration.	Outgo.		107.1		99.7		101.3		113.0
Carb. Dioxide of Res.	Outgo.								
From Comb. of Body Protein.		0.4		0.4		0.2		0.0	
" Storing of Body Fat.			1.8		2.6		2.6		1.7
" Dimin. of Body Water.		**14.3**		**44.1**			**7.2**		**13.4**
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		322.3	322.3	329.2	329.2	286.2	286.2	286.1	286.1

TABLE III.

Balance of Water, Oxygen and Ash.—By Days.

Materials of Metabolism.		Water.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.	921.0		919.0		921.0		923.0	
" Solids.	Income.								
Water of Drink.	Income.	1492.8		1289.9		1299.5		1299.8	
Oxygen from Air.	Income.								
Feces, Water.	Outgo.		77.2		77.2		77.2		77.2
" Solids.	Outgo.								
Urine, Water.	Outgo.		1783.6		1906.0		1440.9		1290.5
" Solids.	Outgo.								
Water of Respiration.	Outgo.		957.9		891.7		905.9		1009.5
Carb. Dioxide of Res.	Outgo.								
From Comb. of Body Pro.									
" Storing of Body Fat.									
" Dim. of Body Water.		**127.8**		**396.0**			**65.6**		**120.1**
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.		277.1		270.0		269.1		274.5	
Loss of Ash.									
		2818.7	2818.7	2874.9	2874.9	2489.6	2489.6	2497.3	2497.3

TABLE III.—*Continued.*

Materials of Metabolism.		Oxygen.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.	818.0		816.2		818.0		819.8	
" Solids.		211.5		211.5		211.4		211.4	
Water of Drink.		1325.8		1145.6		1154.1		1154.5	
Oxygen from Air.		664.3		645.9		642.6		658.8	
Feces, Water.	Outgo.		68.6		68.6		68.6		68.5
" Solids.			7.6		7.6		7.6		7.7
Urine, Water.			1584.1		1692.9		1279.6		1146.2
" Solids.			17.3		17.3		16.8		16.6
Water of Respiration.			850.8		792.0		804.6		896.5
Carb. Dioxide of Res.			604.4		591.6		588.6		600.7
From Com. of Body Pro.		1.4		1.4		0.6		0.1	
" Storing of Body Fat.			1.7		2.5		2.5		1.7
" Dim. of Body Water.		113.5		351.9			58.4		106.7
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.									
Loss of Ash.									
		3134.5	3134.5	3172.5	3172.5	2826.7	2826.7	2844.6	2844.6

Materials of Metabolism.		Ash.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.								
" Solids.		22.3		22.3		22.3		22.3	
Water of Drink.									
Oxygen from Air.									
Feces, Water.	Outgo.								
" Solids.			4.8		4.8		4.7		4.7
Urine, Water.									
" Solids.			19.4		19.4		18.8		18.4
Water of Respiration.									
Carb. Dioxide of Res.									
From Comb. of Body Pro.									
" Storing of Body Fat.									
" Dim. of Body Water.									
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.									
Loss of Ash.		1.9		1.9		1.2		0.8	
		24.1	24.1	24.1	24.1	23.6	23.6	23.2	23.2

carbon dioxid, but a greater difference in the water of respiration and a still greater variation in the urine. The quantity of the latter was so great the first two days that the total amount of water in the body decreased 523.8 g., but during the last two days it increased 185.7 g. The quantity of oxygen derived from the air varies but little, and in nearly the same degree as the evolution of carbon dioxid. The quantity of both oxygen and carbon dioxid was greater on the first and fourth days than on the two intervening days. The ratio of the carbon dioxid exhaled to the oxygen absorbed, called the respiratory quotient, is so important that I have put down the value for each day.

	1st Day.	2d Day.	3d Day.	4th Day.		Total.
Carbon dioxid exhaled,	831.1	813.4	809.3	826.0		3279.9
Oxygen absorbed,	664.3	645.9	642.6	658.8		2611.6
Respiratory quotient,	1.251	1.259	1.259	1.254	Ave.,	1.256

The respiratory quotient for the first day is 0.4% less than the average, corresponding to 2.6 g. of oxygen: that of the second and third days is 0.2% more, corresponding to 1.6 g. of oxygen, while that of the fourth day differs still less, corresponding to only 1.1 g. of oxygen. We know that the respiratory quotient should not be exactly constant from day to day in this experiment, because the receipts and expenditures of oxygen aside from respiration varied slightly. But these variations were only slight, and the nearly constant value of the respiratory quotient is an evidence of the correctness of the values of the oxygen absorbed from the atmosphere, as calculated in the table.

Table IV. gives the balance of matter for each day of the experiment and the calculated gain or loss each day. For the first day there is a discrepancy of 145.4 g. This indicates either a serious error in weighing or else a large error in the water account of that day. For the second day the difference is — 42.6 g., the third day practically nothing, the fourth day — 42.1 g., the total for the four days being 60 g. If the weighings had been more accurate and at exactly 7 o'clock each morning, the result would be more conclusive. But the present purpose is to show how this check may be made in future, rather than to seek to confer any benefits upon past experiments.

TABLE IV.

Balance of Matter.

		1st Day.		2d Day.		3d Day.		4th Day.	
Income.	Food, Water.	921.0		919.0		921.0		923.0	
	" Solids.	552.0		552.0		552.0		552.0	
	Drink, Water.	1492.8		1289.9		1299.5		1299.8	
	Oxygen of Air.	664.3		645.9		642.6		658.8	
Outgo.	Feces.		—		216.6		—		281.8
	Urine.		1855.3		1977.6		1510.6		1358.9
	Water of Respiration.		957.9		891.7		905.9		1009.5
	CO_2 of Respiration.		831.1		813.4		809.3		826.0
	Balance.		**14.2**		**492.5**		**189.3**		**42.6**
		3644.3–	3644.3	3899.3–	3899.3	3415.1–	3415.1	3476.2–	3476.2
	By Scales.	Lost	160.0	Lost	450.0	Gained	190.0	Lost	0.0
	By calculation above.	"	14.6	"	492.6	"	189.3	"	42.1
	Difference.		+145.4		–42.6		–0.7		–42.1

Difference for four days 60 grams, lost more by scales than by calculation.

But one can also calculate the weight of the subject at other times of the day and additional checks may thus be instituted which might be of service in locating errors and deriving the largest possible amount of information from an experiment. Suppose that it is desired to calculate the weight of the subject at the beginning of each 6-hour period, that is at 7 A. M., 1 P. M., 7 P. M. and 1 A. M. of each day. The CO_2 and H_2O of respiration are separately determined for each such period any way, and it is only a matter of slight computation to get all the data for a check upon the work four times each day, having the weight of the subject by an accurate scale at the same times.

In computing the fat gained or lost from the gain or loss of carbon, it is assumed that the glycogen and other carbohydrates of the body at 7 A. M. each day are constant in quantity, the diet being uniform from day to day. In order that the same assumption might hold true, or approximately so, for the six-hour periods it would be desirable to serve the meals just after the beginning of the periods. Then the glycogen content of the liver and tissues would be more nearly uniform than if supper were served at 6:30, as was done in these experiments. Even then, of course, it would vary, but the error introduced by the probable variation would not be considerable.

TABLE V.

Table of Gain and Loss.

	1st Day.		2d Day.		3d Day.		4th Day	
	Gain.	**Loss.**	**Gain.**	**Loss.**	**Gain.**	**Loss.**	**Gain.**	**Loss.**
Water.		127.8		396.0	65.6		120.1	
Feces.	106.0			110.6	106.0			175.8
Protein.		5.7		5.7		2.5		0.5
Fat.	15.2		21.7		21.4		14.4	
Ash.		1.9		1.9		1.2		0.8
Balance.	**14.2**		**492.5**			**189.3**	**42.6**	
	135.4	135.4	514.2	514.2	193.0	193.0	177.1	177.1

In the accompanying figures a graphic representation of the change of weight of the person during the 96 hours of the experiment is given. The points representing the weights at the end of every 6 hour period are inclosed in small circles, and the weight as computed is given in figures. Between these points the values are not exact, for it has been assumed that the evolution of H_2O and CO_2 and the absorption of oxygen were uniform during each period. Of course these quantities are greater during the exercise incident to dressing and eating than during subsequent rest, but for the purpose of illustrating the fluctuation of weight during a day, or during four successive days, this variation may be neglected. It would be of very great interest if we had the precise weight of the subject at each of these 17 six hour stations. Another source of uncertainty in the calculated weights at the six hour stations is that the respiratory quotient, determined above for each day separately, was assumed to be constant for the different periods of each day. But in experiments carried out with that in view it might be separately computed for each period. That might give information of value in itself as showing to what extent oxygen may be stored up in the body.

The chart is mainly self-explanatory. Starting at 7 o'clock on the morning of the first day with a weight of 68,420 g. the subject loses 45 g. in one hour by respiration. That is, the loss of CO_2

and H_2O above the gain of oxygen by respiration is 45 g. per hour during the first 6 hours. Breakfast adds 675 g. to his weight.

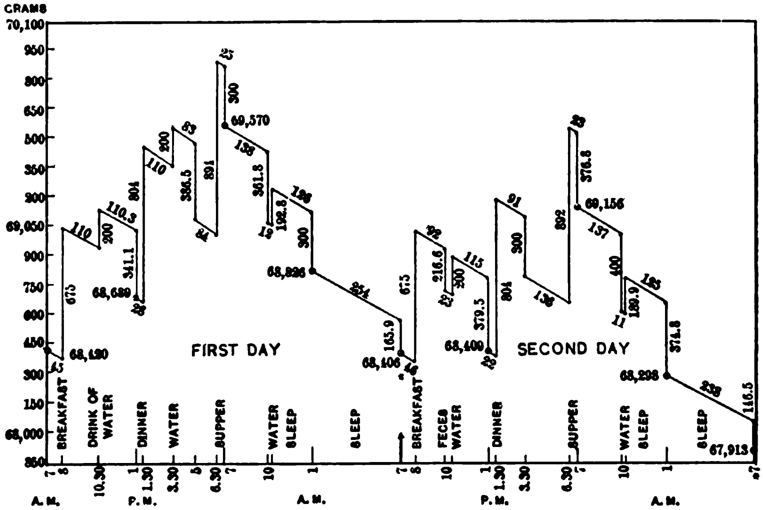


Fig. 1.

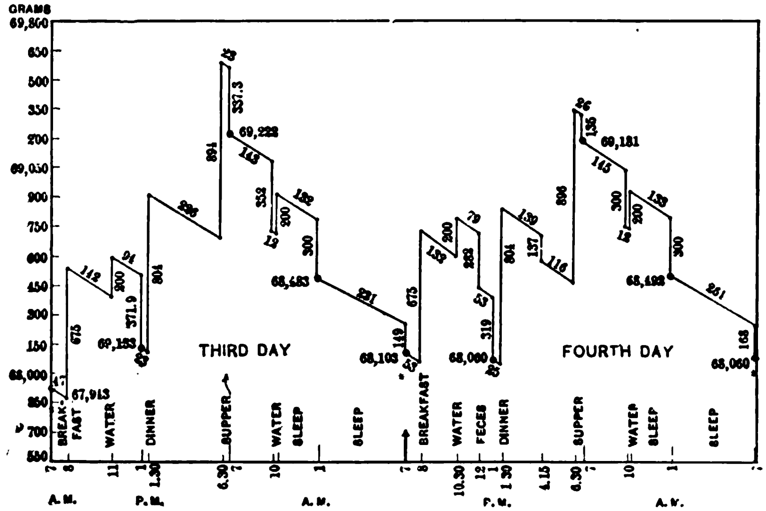


Fig. 2.

Of course the gain of weight due to breakfast was not instantaneous, but it was not deemed necessary or desirable to indicate the time occupied in eating meals by a sloping line. During the next 2½ hours until 10:30 a loss of 110 g. occurred by respiration, and then a drink of water increased the weight by 200 g. A further loss of 110.3 g. by respiration was followed by a decrease due to passage of urine, and at one o'clock the weight should have been 68,689 g. We have no observed weight to compare with it, but the weights we have at seven o'clock each morning are plotted in the chart as stars.

The charts show very clearly the considerable fluctuations of weight throughout a period of 24 hours. The minimum weight is at eight o'clock, just before breakfast, and the maximum just after supper. The slope of the lines showing the loss of weight by respiration is nearly uniform, as the loss of weight is nearly as rapid at night during sleep as during the day. The evolution of CO_2 is only slightly more than half as much at night as during the day (and in a work experiment only about one-fourth) but the absorption of oxygen is also less in (nearly) the same ratio. On the other hand, the rate at which water is given off from the skin and lungs is nearly as great at night as in the day in a rest experiment (although far less at night than during the day when at work). Hence while there was a loss of 265 g. by respiration in 6 hours, from 7 a. m. to 1 p. m. on the first day, there was a loss of 254 g. from 1 a. m. to 7 a. m. during sleep.

While the check that is thus afforded by knowing the weight of the subject at intervals during the experiment would not detect small errors in the determination of nitrogen and carbon, it would detect appreciable errors in the determination of the water, and this is the quantity most in need of such a check. Considering the great labor and cost of complete respiration experiments it appears of the greatest importance to use every possible means to reduce errors to a minimum and to derive the maximum amount of information from each experiment.

In order to weigh the subject with the greatest possible accuracy, he should be seated quietly on the platform of the balance, and the weighing done from without. This may be accomplished by using a rider, controlling it by a rod from outside, in a manner similar to that in a chemical balance. The subject would set the heavy weight at such a point that the beam nearly balanced. The exact balance would then be secured by the rider. The beam should be visible from without through the window, so that readings of the weight and rider could be made by the observer outside. The subject ought, however, to note and report the reading, when the weighing is finished, as a check upon the record of the observer. The position of exact balance could be obtained by use of a reading telescope, set in the walls of the chamber and a graduated scale attached to the beam would swing to and fro in the field. By observing the range, very close settings could be made, and with a good Fairbanks balance readings could be made at least to a single gram.

In order that these close readings be as reliable as possible, several successive weighings should be made. The subject is constantly growing lighter by the losses due to respiration. Hence the rider could be set a little to one side of a balance, and the time noted when exact balance was secured. A second setting would give a balance, say about a minute later. Let five such weights be made, extending before and after the time at which the weight is desired, and from the mean the true weight could be obtained to within a gram at least, not counting any constant error due to inaccuracy of the balance, which would be of no moment in taking the difference.

The subject should have a light suit of clothes, without pockets, and be weighed with his clothes on. Any moisture contained in his clothing would then be reckoned as part of himself. In a rest experiment this would be small, and the disturbance due to the exercise of dressing and undressing would be avoided.

MECHANICAL WORK.

The bicycle-dynamo combination, which we employed to develop and measure mechanical energy at the expense of the potential energy of food and tissue was not the ideal form of ergometer. It was easily arranged and served our purpose for the time being. But it required calibration and the energy expended in friction was somewhat variable; moreover, it is probably not a machine in which the maximum efficiency of conversion could be effected. A better arrangement would be a simple treadmill. That is, a movable inclined plane consisting of slats attached to two endless chains, the latter running over sprocket pulleys. A fly wheel to steady the motion might consist of a heavy copper disc revolving between the poles of permanent magnets, thus serving also as a brake. By varying the position of the magnets the braking effect and, therefore, also the rapidity of motion could be varied, and by varying the inclination of the plane greater or less force would be exerted. The three quantities required in order to calculate the work done in any given time would be (1) the angle of elevation of the plane, (2) the whole number of revolutions of the flywheel or sprocket pulley and the distance traveled by the plane for each such revolution, and (3) the weight of the person operating the mill. The first is easily determined by reading a graduated sector, the base being level ; the second would be recorded by a simple counter; the third should be frequently determined for purposes of a check upon the observations and analyses, and hence would not involve any additional labor. This form of ergometer would enable a large or small amount of work to be done, would give a high efficiency of conversion, would require no calibration, would involve no error due to varying friction, would require no record of varying speed to be made, and could be constructed at a reasonable cost. With it a large variety of experiments could be made.

The conversion of the energy of food and tissue into mechanical energy is a profound mystery. We cannot understand how a contracting muscle transfers the energy liberated by the oxidation of matter into an outside portion of matter. Perhaps the heat produced by the oxidation is transformed by some process equivalent to that of a heat engine. If the second law of thermodynamics does govern the process, it may be that the requisite difference of temperature occurs only in spaces of the order of magnitude of molecules, and hence cannot be measured experimentally. Or it may be that the oxidation is accompanied by an evolution of less heat when work is done, as is the case when zinc is consumed in a battery furnishing a current of electricity. In other words, that it is in some way electrical, and hence that no such difference of temperature exists as would be required in a heat engine or its equivalent. Some such theory has been put forward, although it is far from explaining the process. It is possible that further experiments such as can be carried on with the respiration calorimeter will give such additional information as at least to assist in formulating a more satisfactory theory of the process.

But there is another case of interest which demands experimental examination. That is the converse case where work is done upon a man, for example, by an outside agent. If a laborer spends a day in hoisting brick to the top of a building by means of a windlass, the heat which his body gives out plus the work done upon the brick will equal the net potential energy of food and body tissue used up in the same time. Now suppose that next day he lowers the same brick to the ground again. The work done is negative in the mechanical sense, although the force exerted in handling the windlass is nearly as great as in hauling them up. Will the heat evolved *minus* the loss of potential energy of the brick equal the potential energy of food and body tissue used up? And will the heat yielded by the body be more or less than on the preceding day?

Or, again, suppose the windlass is driven by an engine while the laborer grasping the handle holds back with all his might. The engine does a large amount of work upon the man. What is the form that this energy assumes? Is oxidation checked by the exertion of force upon muscle?

Or, again, suppose a man is walking down hill. If he weighs 150 pounds and walks 10 miles down a grade of 14½° he will descend vertically 2½ miles and the potential energy given up by his body as a whole will be 1,980,000 foot pounds, which is equivalent to a horse-power working for an hour. What form does this energy take? Such an experiment could be performed in a respiration calorimeter by using the proposed treadmill for an inclined plane, and driving it by an electric motor, having the man walk down hill as the plane was driven up. Perhaps such experiments have already been made, but so far as I am aware no satisfactory answers to these questions have been reached.

An intermediate case is where force is exerted without doing work, as when a weight is held in the hands without being raised or lowered.

The suggestion that there is an energy-bearing product of mental effort which is capable of being apprehended by physical instruments is contrary to our physical notions. The increased generation of heat in the brain and head, due to increased circulation of the blood or increased oxidation of tissue or other oxidizable material during mental activity is however something that can be studied. But it would require special apparatus other than, or in addition to, the respiration calorimeter.

Wesleyan University, Middletown, Conn., September 5, 1899.

A DISSOCIATION THEORY OF THE ELECTRIC ARC.

By C. D. Child.

A SERIES of very instructive photographs of the alternating current arc were shown by Mr. N. H. Brown in the Physical Review for November, 1898. An experiment recently performed by the present writer bears somewhat upon some of the phenomena there noticed and it may not be amiss to review briefly the results of Mr. Brown's article before describing this experiment. In order to refer more easily to the photographs Plate II. of that article is here reproduced. These are photographs of the alternating current arc taken upon a rapidly moving film. A screen with a narrow slit was so placed that light from but a small section taken lengthwise of the arc fell on the film. The films moved from left to right, and accordingly when read from right to left, they give the history of the arc through successive alternations. There appear to be two light fronts, one starting somewhat sooner than the other. As indicated by Mr. Brown, it is the negative one which starts the sooner, but moves with the slower velocity. The different photographs were taken with different velocities of the film and with different distances between the carbons but the general characteristics are the same in all here reproduced.

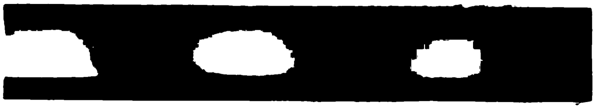
There would seem to be no doubt as to the cause of the fact that the arc starts at the negative carbon. As was shown the carbon which is negative at the beginning of a passage of the current was in the previous discharge positive, and it is well known that after the current has once been established the positive carbon is the hotter. This carbon would continue to be the hotter after the previous discharge had ceased, the vapors in its immediate vicinity would continue to be hotter than elsewhere and the current would accordingly begin at this point.

The explanation of the fact that the positive light front appears to move the more rapidly is not so simple. But though not simple, it may be the means of giving us a much better knowledge of the arc than that which we now possess. The explanation was suggested that the current was at least in part carried by ions and that the positive ions move with the greater velocity. Mr. Brown does not appear to consider this a probable explanation, and in fact it does not seem to be a possible one in the form suggested, *i. e.*, that the greater velocity of the positive ions may be due to the greater fall of potential at the positive carbon. For the distance through which the positive ions have the greater velocity is shown by the photographs to be greater than the space in which the fall of potential is so great and millions of times greater than the mean free path of the molecules at atmospheric pressure. We cannot see how the mean free path of the ions carrying the current can be greater than that of ordinary molecules. The ions will not maintain their velocity more than the distance of their mean free path beyond the space where the fall of potential is so great, unless the air move as one mass, but the photographs show that there is a motion from the negative to the positive as well as from the positive to the negative, and the air, as a whole, cannot move in both directions at once.

There is, of course, the possibility that the effects shown in the photographs are not caused by ions of molecular size, but particles of carbon which are driven off. But if this were the case, we should expect a disturbance of some kind at the point where the streams of negative and positive particles meet, and that this would be shown in the photographs. Nothing of this sort, however, is noticeable.

There are indications in the photographs of a phenomenon which would afford a still stronger reason against the idea that the greater velocity is caused by the greater fall of potential at the positive carbon. In one or two cases, especially in the discharge shown at the left hand of *2A* the negative appears to move slower at the same time and in the same region where the positive front is moving more rapidly. If this should be shown to be the case, it would afford very strong evidence that the current is carried by ions, and that the positive ones move the more rapidly, not because of the greater fall of potential at the positive carbon, but because they do so even with the same potential gradient.

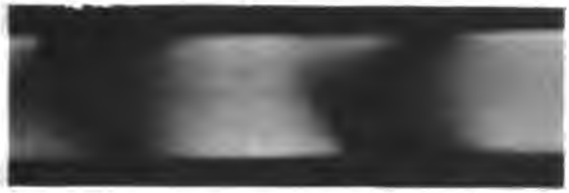
It is not known to the present writer whether it is possible to


2A

2B

2C

2D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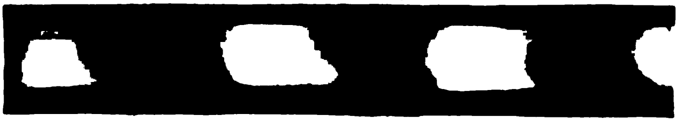
2F

N. H. BROWN. PHOTOGRAPHIC STUDY OF THE ELECTRIC ARC.

[To accompany an article by C. D. CHILD.]

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make photographs of such a character as to decide definitely this point, and in the absence of such knowledge it seemed well to consider the conclusions to which such a hypothesis would lead. Moreover the experiment to which reference has been made tends to substantiate this view. The following article will accordingly be devoted, first, to a discussion of the hypothesis that the current is carried in the arc by ions and that the positive ions with the same potential gradient move the more rapidly; and second, to a description of the experimental work performed.

As is well known, this resistance of the arc is not an ohmic resistance. A minimum difference of potential is necessary in order to maintain an arc, and when the current is increased the apparent resistance becomes smaller. Indeed, the resistance decreases so rapidly in the case of poles of solid carbons with a definite distance between them that the difference of potential becomes smaller as the current becomes greater.[1] The greater part of the fall of potential through the arc occurs at the surface of contact between the conducting gas and the carbons, and by far the greater fall is at the positive pole. Diagram 1 represents the fall of potential between two poles according to the data found by Mrs. Ayrton.[2] The continuous line would represent the case when a current of 10 ampères flows between the solid carbons at a distance from each other of 8 cm. The explanation often offered is that there is a counter E. M. F. in the arc caused by a combination of the Thomson and Peltier effects. However, it has never been possible to find any trace of the counter E. M. F. after the impressed E. M. F. has been removed. This is surprising since the gases of the arc can be shown to be conducting for an appreciable time after the current has ceased to flow, and a counter E. M. F. acting for 1/200 sec. would surely be detected.[3] If the effect is a true Thomson effect, it will last as long as the inequalities of temperature continue. That the inequalities do last for an appreciable time is shown by photographs of the arc. There may, of course, be some action similar to the Thomson effect but in just what the similarity consists has not yet been shown.

[1] Lond. Elec., Vol. 34, p. 366.

[2] Lond. Elec., Vol. 41, p. 720.

[3] Lond. Elec., Vol. 39, p. 615.

On the other hand the suggestion that the current in the arc is carried by ions and that the positive ions have the greater velocity would appear to offer a satisfactory explanation of the arc. A similar hypothesis was given by Prof. Zeleny to account for the phenomena in the case of discharge produced by X-rays.[4] His hypothesis needs but little modification to adapt it to the case in hand. The principal difference would be that in the case described by Prof. Zeleny the negative ions had the greater velocity, while in the case in hand the positive ions have the greater velocity.

We must suppose that the intense heat of the arc dissociates the gases of the arc into positive and negative ions. We cannot at present know whether the charges on the oppositely charged ions are or are not equal. But in any case it would not alter the essential idea of this article, and for simplicity we may assume an equal number of positive and negative ions with equal charges on the two kinds of ions. On being dissociated the positive ions move toward the negative pole, and the negative ions towards the positive pole. No doubt recombination and again dissociation take place frequently. But however often this may occur there will be an action equivalent to a motion of the negative ions in one direction and the positive in the opposite direction.

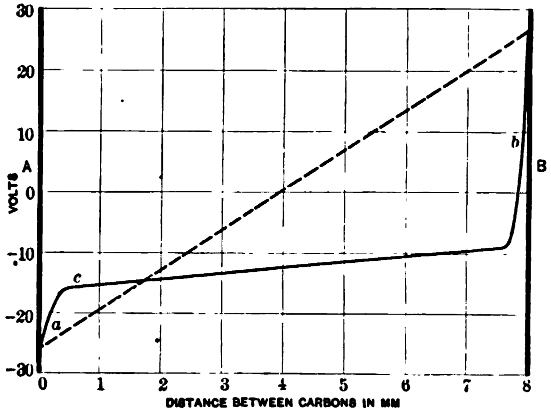

Fig. 1.

There would be an excess of positive ions in the vicinity of the negative pole and of the negative ions in the vicinity of the positive pole somewhat as indicated in Fig. 2, where the negative pole is at the left. The amount of electrification is shown by the ordinates. This electrification would produce an effect on the potential. Without any electrification the potential would fall regularly from one pole to the other as shown by the dotted line in Fig. 1. The electrification would change it so as to conform roughly with the continuous line in Fig. 1.

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[4] Phil. Mag. (5), Vol. 46, p. 120.

If now the positive ions move with the greater velocity, they would in general take less time in reaching the pole toward which they are moving, and there would consequently be fewer of them between the poles at any instance of time. In a similar way if two armies having equal numbers of soldiers were to march over a given distance, the army moving the more rapidly would afford fewer soldiers per acre. The same is true of the ions. This will be true whether the ions move directly across from pole to pole, or only succeed in going across after many interruptions produced by their dissociating and recombining. The excess of negative ions is indicated in Fig. 2. If then there are more negative than positive ions between the carbon terminals, they would cause the potential of the gas to be lower than it otherwise would be, as is indicated in Fig. 1.

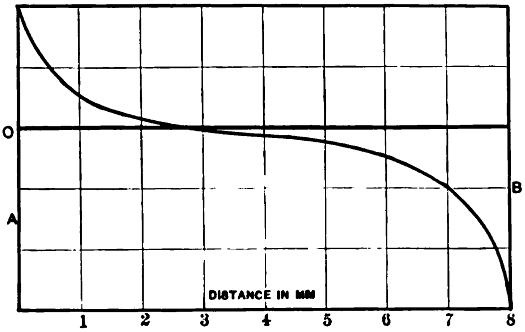

Fig. 2.

At the surface of contact between the gases and the carbons the conditions are somewhat exceptional. At *a*, Fig. 1, there would be no negative ions moving toward the right. If the potential gradient were the same here as elsewhere, there would be positive ions at *c*, which had come from the right, but no negative ions from the left to neutralize the electrostatic effect. The potential at *c* would, therefore, become positive, due to the positive ions, and the rise of potential between *a* and *c* would be greatly augmented. A similar effect would occur at the positive pole. This would account for the large fall of potential at the poles. The fact that the potential of the gas is lowered, because of the greater velocity of the positive ions would make the fall of potential at the positive pole greater than at the negative.

It is impossible to put this explanation into mathematical form, because of the complexity of the conditions. The amount of dissociation at any point depends on the temperature at that point according to some law which is not at present known, and the temperature in turn depends on the potential gradient which in turn depends on the amount of dissociation. It would be wiser to leave this circle of dependencies until our knowledge of the subject is in a more advanced state.

With this explanation of the arc it is easy to see why the apparent resistance becomes less as the current is increased. With increase of current there would be more heat developed in the arc. A greater dissociation would occur, and this in turn would cause the apparent resistance to be less. It would, however, be impossible for the potential difference to become smaller so fast that the amount of heat developed by a large current would be less than that developed by a smaller one. As far as is known this does not occur.

This view also harmonizes with the fact that with an alternating current the average potential between the poles is not zero, but is negative. The positive ions moving with greater velocity pass out of the field quicker, leaving an excess of negative ions and a corresponding negative potential.

The experiment referred to at the beginning of this article was suggested by a recent article in the *Philosophical Magazine*[1] by Professor A. P. Chattock on the velocity of ions in the case of discharge from a point to a plate. From his experiment there appears to be a drag produced on the air by the friction between the air and the ions, and this drag shows itself as an increased air pressure at the plate. Professor Chattock was able to measure this pressure by a sensitive manometer. It seemed possible to the present writer that if the current of the arc is carried by ions their presence could be detected by a similar method.

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[1] Phil. Mag. (5), Vol. 48, p. 401.

The manometer used in this experiment was similar to one shown me at one time by Mr. W. R. Turnbull. I am unable to state whether any description is given of it elsewhere or not. It consists of an air-tight chamber, 4 cm. on each side and 1 cm. thick. One side is covered with rubber dam which moves with a very slight increase of pressure on the air within the chamber. The chamber is shown in cross section in Fig. 3. *A* is the chamber which may be connected at *e* to the other parts of the apparatus. *R* is the rubber dam covering one face. In front of the rubber is the mirror *m*. The mirror is fastened to the needle *n* which is free to rotate. *a* and *b* are strips of tin projecting from the chamber *A* and holding the needle *n* in place. The wire *d* is fastened to the rubber dam and also to the mirror near to the axis about which the mirror rotates, so that a slight motion of *d* gives the mirror a sensible rotation. This rotation was read by a telescope and scale. It has not as yet been possible to do any quantitative work, and accordingly the manometer was only roughly calibrated by comparing it with a water manometer. A pressure of 1 mm. of water corresponded to a deflection of 10 cm. with the sensitive manometer. A deflection of 1 cm. would therefore correspond approximately to a pressure of 1/100,000 of an atmosphere.

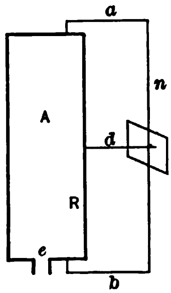

Fig. 3.

A small hole was drilled lengthwise through one of the carbons and the manometer was connected with this aperture.

On starting the arc the manometer showed an increase of pressure. It was, of course, impossible to keep the end of the arc on the same point on the carbon, and as a result there was a continual variation in the pressure indicated. The pressure was greater as the end of the arc was nearer the aperture. This continual variation of the pressure made any qualitative measurement impossible, but it was plain that the pressure was greater at the positive carbon.

In order to remove any doubt a manometer was constructed so as to measure the *difference* between the pressures at the positive and the negative carbons.

The instrument was the same as that shown in Fig. 3, except that in front of *R* was placed a second air-tight chamber. A window in this chamber allowed the motion of the mirror to be detected as before. One chamber was connected with the aperture through one carbon, and the other chamber to the aperture of the other carbon.

With this instrument the deflection was to the right or to the left depending on the direction in which the current passed between the carbons. The deflection was always reversed when the current in the arc was reversed. By this it was proved that the pressure at the positive carbon was greater than that at the negative. The deflection of the manometer was at times as much as 5 cm., showing that the difference of pressure was .00005 of an atmosphere.

The effect was very much more marked with a large current than with a small one. Thus when the current was 6 ampères there was scarcely any pressure indicated. When the current was 20 ampères an average reading of the manometer would indicate a difference in pressure of .00002 of an atmosphere. This was possibly due to the fact that the arc in the latter case covered a larger part of the carbon about the aperture.

The carbons were placed horizontal in order to avoid convection currents. But when they were placed other than horizontal no difference in the effect was noticed. Moreover, it seems impossible that the effect could be due to convection currents, because reversing the current without in any way changing the position of the carbons or their connections to the manometer would reverse the effect.

The effect may be due to the expansion of the gases in the arc caused by the heat, perhaps to the formation of CO_2 or to vaporization of the carbon. We should in this case expect the pressure at the positive carbon to be the greater, because the arc there is the hotter. This is, indeed, what we find. There is one objection to this view. If this effect were due to the heat of the arc, one would expect that when one carbon was directly over the other the upper carbon would show the greater pressure whichever way the current flowed. This was not found to be the case. The positive carbon showed the greater pressure whether it was up or down. Moreover, the experiment was tried of inclosing the carbons in a chamber from which free access of air was excluded as far as possible without stopping all vent for the expansion of the gases caused by the arc. The effect was in this case the same as when free access of oxygen was allowed. These cannot be considered as proofs that the effect is not due to differences of temperature, but they are at least facts which will warrant us in considering some other hypothesis.

Professor Chattock showed that in the case of discharge from a point to a plate $p = \frac{Cz}{V}$ where p is the total drag exerted on the air, C equals the total current, z equals the distance between the point and the plate and V equals the velocity of the ions for unit potential gradient. In the case of the arc the conditions are much more complicated. Instead of the current being all carried by ions of one sign, it is carried by both positive and negative ions. This in itself would not make the problem impossible of solution, if at all points the same part of the current were carried by ions of one sign. This, however, is not the case, and because of irregularities of the arc it is impossible to make any mathematical statement which will apply. But we can get some insight into the matter by considering the physical meaning of the equation $p = \frac{Cz}{V}$. The drag which the ions produce must be equal to the force exerted upon them since their acceleration is negligible. It is as it would be with a block being pulled over the floor with a uniform velocity. The pull on the floor due to the friction is the same as the pull on the block. So the drag on the air is equal to the total attraction between the plate and the ions which are at any instant above the plate. This will evidently be greater the greater the number of ions. Other things being equal the number of ions will be greater the slower they move. This is shown in the equation $p = \frac{Cz}{V}$. The number will be greater the more current is being carried and will be greater the greater the distance between the point and the plate. The drag will, therefore, be proportional to C and to z.

With this conception of the cause of the drag, *i. e.*, that it is caused by the attraction between the electrodes and the ions, we can apply it qualitatively to the arc. Referring to Fig. 2 we see that there must be a pressure on the positive carbon due to the attraction between the carbon and the negative ions near it. There will also be a pressure on the negative carbon for a similar reason. The attraction on the positive carbon will, however, be the greater because there are more negative ions between the carbons than positive ones. This, indeed, was what was found in the experiment.

It is thus seen that all the phenomena of the arc which are known can be explained in accordance with the hypothesis here suggested. The action of the arc is, however, very complicated, and it may well be that, though this explanation be partially correct, it is not a complete explanation. It is hoped that a more thorough investigation of the phenomena here noticed may bring to light facts more decisive, and result in definite knowledge concerning what has heretofore proved a troublesome problem.

Colgate University, January, 1900.

THE EFFECT OF MAGNETIZATION ON THE ELASTICITY OF RODS. II.

By James S. Stevens.

In the Physical Review for August, 1899, there was described a series of experiments in which by the use of interference methods, it was shown that iron and steel rods developed a greater elastic modulus when magnetized. In those experiments flexional elasticity alone was considered. In the present paper I shall describe some experiments which had for their object the determination of the effect of magnetization upon torsional elasticity. That some relation exists between torsion and elasticity has been known since 1847, when Matteucci examined the change in magnetization undergone by an iron rod when it was twisted. Further investigations have been carried on by Wertheim, Wiedemann, Kelvin and others, with the general conclusion that when a rod of soft iron

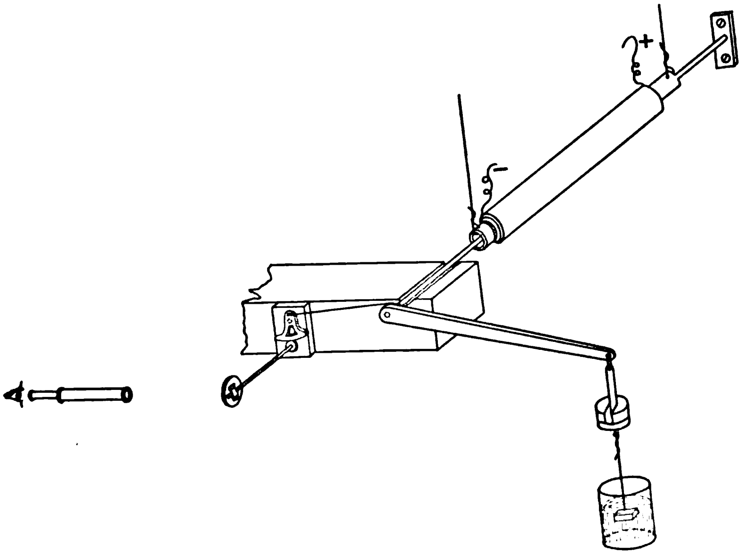

Fig. 1.

exposed to longitudinal magnetizing force was twisted, its magnetism was reduced by torsion in either direction (Ewing).

In these experiments three styles of cylindrical rods were employed: one of iron, 183 cm. long and 0.48 cm. in diameter; one of steel of the same dimensions, and lastly the iron rod was reduced in length to 163 cm. To magnetize these rods a coil of two layers of No. 16 wire was wound in 1,612 turns. The length of the coil was 132.1 cm. One end of the rod was rigidly screwed into an ron plate, fastened to a solid wall, and to the other was attached an arm carrying a weight at the opposite end which supplied the force of torsion. The arrangement of the apparatus is shown in the accompanying figure.

After the rod had been twisted by the application of the given weight the current was sent through the coil, and in every case where the current was sufficiently strong a *rising* of the weights was observed, showing that the magnetizing effect was such as to increase the torsional elasticity. In order to damp the vibrations a small weight suspended in a jar of water served as a dash pot. The following table shows approximately the angles through which the rods were twisted by the loads applied.

Moment of Torsion (dynes).	Angle of Torsion (degrees).
6.3×10^6	2
4.8×10^7	15
9.8×10^7	30
13.6×10^7	39

To measure the change which took place in the angle of torsion when the rods were magnetized, I employed two methods: first, a mirror was fastened to the arm and the rotation of the rod was measured by a telescope and scale; secondly, in case the rotation was small, a device shown in the figure was employed which multiplied the scale readings 9.43 times.

In the following tables are shown the current, magnetizing force, scale readings and the angle representing the corresponding rotation of the rods.

TABLE I.

Iron rod, 183 *cm. long. Moment of Torsion* 9.8×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
7.7	0.01	0.3	50.0	0.94	25.5
13.7	0.02	0.6	54.6	0.97	26.3
19.9	0.12	3.3	71.9	1.1	33.0
23.0	0.29	7.8	76.5	1.2	36.0
29.1	0.45	12.3	79.6	1.2	36.0
32.1	0.66	18.0	91.8	1.2	36.0
35.2	0.72	19.2	104.0	1.3	39.0
38.3	0.74	20.1	117.8	1.3	39.0
42.8	0.83	22.5	127.0	1.3	39.0
44.3	0.87	23.7	137.7	1.4	42.0

TABLE II.

Iron Rod, 183 *cm. long. Moment of Torsion* 4.8×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
4.6	0.01	0.3	36.7	0.90	24.6
9.2	0.02	0.6	39.8	0.92	25.2
13.8	0.23	6.6	47.4	1.00	27.3
16.8	0.35	9.6	52.1	1.06	28.8
19.9	0.46	12.6	64.3	1.13	30.9
23.0	0.62	16.8	76.5	1.22	33.3
26.0	0.69	18.9	82.3	1.23	33.6
30.6	0.78	21.3	90.3	1.25	34.2
32.1	0.80	21.9			

TABLE III.

Steel Rod, 183 *cm. long. Moment of Torsion* 9.8×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
7.7	0.1	2.6	76.5	0.2	5.2
15.3	0.1	2.6	91.8	0.3	7.8
22.9	0.1	2.6	137.7	0.3	7.8
30.6	0.1	2.6	145.4	0.3	7.8
35.2	0.1	2.6	153.0	0.4	10.4
38.3	0.2	5.2	168.3	0.4	10.4
58.1	0.2	5.2	214.1	0.5	13.0
61.2	0.2	5.2			

Table IV.

Iron Rod, 163 *cm. long. Moment of Torsion* 9.8×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
12.4	0.05	1.3	79.4	0.4	10.0
19.9	0.15	3.8	107.1	0.4	10.0
27.5	0.2	5.0	130.0	0.5	12.5
30.6	0.2	5.0	137.7	0.5	12.5
38.3	0.3	5.0	153.0	0.5	12.5
61.2	0.3	7.7	183.6	0.6	15.1
68.8	0.4	10.0	198.9	0.6	15.1
76.5	0.4	10.0			

Table V.

Iron Rod, 183 *cm. long. Moment of Torsion* 13.6×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
33.7	0.01	0.6	91.8	0.67	18.3
47.4	0.04	1.2	183.6	1.14	28.2
53.6	0.08	1.8	229.5	1.14	31.2
56.6	0.26	7.2	306.0	1.17	31.8
64.3	0.48	13.2	382.3	1.19	32.4
82.6	0.58	15.9	405.5	1.22	33.3

Measurements were also taken with no weight added to the arm of torsion except the small one used with the dash pot. The moment of torsion was 6.3×10^6 dynes. While a deflection was noted with each magnetizing force used, the scale readings were quite irregular and the zero not constant.

By use of the device for multiplying the scale readings, it is thought that the error in estimation need not exceed eight seconds.

The above curves were plotted with magnetizing force divided by 4 and corresponding angles of rotation expressed in minutes as coordinates. *I* is for the steel rod, *II* the short iron rod, *III*, *IV* and *V*, the longer iron rod with moments of force 13.6×10^7, 4.8×10^7 and 9.8×10^7 dynes respectively. Without doubt, the lack of regularity of the lines is due in large measure to errors of observation.

From the general consideration of the experiments, and the inspection of the tables and plots, the following conclusions may, I think, be written down :

1. Magnetization of an iron or steel rod increases its torsional elasticity. This would be expected from the results of the experiments mentioned in the first part of this paper dealing with the effects of torsion upon elasticity.

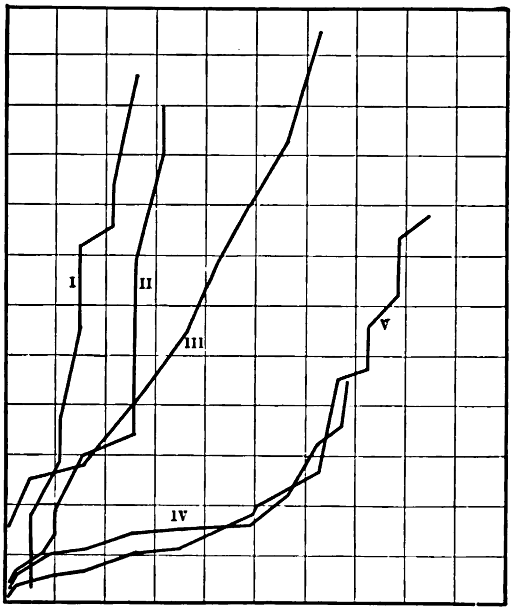

Fig. 2.

2. The effect is greater in iron than in steel rods of the same dimensions.

3. The increase in elasticity varies with the length of the rod.

If we compare the results of this experiment with those of the experiments on elasticity of flexion, we note a distinct agreement. In the first place the modulus of elasticity is increased in each case when the rod is magnetized ; the increments are fairly proportional to the magnetizing forces ; and they are inversely proportional to the magnitudes of original stresses. So far as elasticity of traction is concerned, the only results published are given in the Physical Review, Volume II., No. 4. They do not represent exact measurements but are inferences which were suggested in connection with another experiment, and go to show that in this case also magnetization increases the elasticity. It is expected that a little later the effect of magnetization upon a rod subjected to longitudinal pressure will be studied.

It ought to be said that the mechanical arrangements for this experiment are due to H. G. Dorsey, and that in taking the readings I was assisted by S. Sidensparker, instructors in the Department of Physics.

Physical Laboratory, The University of Maine.

THE INFLUENCE OF TEMPERATURE, PRESSURE, USED SOLUTIONS, AND SIZE OF ANODES ON THE DEPOSIT OF SILVER VOLTAMETERS.

By J. F. Merrill.

THE object of the work herein described was to see if by a change of temperature or pressure, etc., the electric charge carried by silver ions in the electrolysis of silver nitrate could be varied. It is well known that the amount of the silver deposit, as obtained in ordinary electrolysis, varies slightly according to the conditions under which the deposit is obtained. But that this variation is due to any change in the carrying capacity of the silver ions has probably not been proved.

The method here employed was of course comparative. Two silver voltameters, usually alike in all respects, were placed in series and the deposit from one under ordinary conditions compared with the deposit from the other obtained under different conditions. Then any differences in the two deposits must be due to the different conditions. After formation, the two deposits were treated alike with respect to washing, heating, etc., to avoid any variation in amount due to different treatment.

Influence of Pressure.

Preliminary experiments having shown that the pressure must be considerable to produce any certain effect, a piece of apparatus was made by means of which the voltameter could be subjected to a gaseous pressure up to 120 atmospheres. The pressure cage was made by boring a hole 3¼ inches in diameter in a piece of steel shafting, threading a portion of it, and then screwing it down upon a suitably prepared cast-iron base up through the bottom of which the conducting wires passed and the compressing gas was admitted.

The Rayleigh form of silver voltameters was employed. The diameter of each platinum dish, or bowl, was 7 cm., and the dimensions of the silver anodes, when new, were 4 cms. diameter by 2 mm. thick.

The insulation of the voltameter in the cage seemed to be perfect, but, to make sure of no error due to leakage, the position of the two voltameters with respect to the battery was reversed in alternate experiments.

The pressure was secured by admitting compressed air from a cylinder into the cage.

The following results were obtained :

No. Experiment,	1	2	3
Pressure in atmospheres,	103	95	103
Deposit in air,	1.1371	1.23745	1.08205
" under pressure,	1.1370	1.23740	1.08190
Difference in grams,	0.0001	0.00005	0.00015

The time of deposit in each case was about 32 minutes. According to these results, no pressure effect seems to have been produced, as the differences, though all in the same direction, are probably within the limit of experimental error.

To see if the air absorbed under pressure exercised any influence, two deposits were made after the voltameter had stood, in each case, during several hours under a pressure of 90–100 atmospheres. The differences in the deposits as obtained in air at ordinary pressure and under the hood, were 0.00000 and 0.00010 grams, respectively.

But it has been observed[1] that gases dissolved in the silver electrolyte affect the amount of the deposit. The result here reported may, however, be reconciled with these observations if it is remembered that oxygen and carbon dioxide decrease and nitrogen increases the deposit. Using air N, therefore, neutralizes the effect of O and CO_2.

Effect of Temperature.

In testing for a temperature effect, experience suggested a form of voltameter much more convenient for the purpose than the Rayleigh form. The electrolyte was contained in a glass beaker on the bottom of which rested the silver anode, while the disk-form kathode was suspended horizontally near the top of the electrolyte, just as the anode ordinarily is. The kathode was made of platinum wire 0.6 mm. in diameter, wound about cross-arms like the bottom of a basket into a close, flat spiral with the suspension wire coming out from the center perpendicular to the plane of the spiral. (If the electrolyte is a water solution a disk of sheet platinum makes a good kathode.) The current was led down to the anode by a plantinum wire passing through a small glass tube. In this form of voltameter no filter paper wrapping for the anode is necessary, and, for small currents, it is a cheap and satisfactory form in practice.

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[1] Schuster and Crossley, Proc. Roy. Soc., 40, p. 344, 1892, also Meyers, Wied. Ann., 55, p. 288, 1895.

Here again the two voltameters were alike and were placed in series. The silver nitrate solution in one was boiled and then cooled in the same beaker to the temperature of melting ice, and the deposit from this compared with that obtained from a solution maintained at about 90°C. The object of boiling one solution and then cooling was so that both solutions should have nearly the same degree of freedom from dissolved gases.

In these experiments the value of the current was 0.25–0.3 ampère, and the area of each kathode about 13 sq. cm. The weights of deposits from five consecutive tests follow:

From hot solution,	0.79835	0.8434	0.82655	0.86470	0.85900
From cold solution,	0.79820	0.8435	0.82655	0.86420	0.85900
Difference,	0.00015	—0.0001		0.00050	

The average difference in these deposits is again within the limits of experimental error. But this result is apparently not in agreement with that of four observations made by Rayleigh, who found a difference of about 1 part in 2,500 in favor of the deposit from a solution at 50° C. as compared with that obtained from a solution at 4° C.; but it is in agreement with the result of two observations made by Kahle.[1] But if the deposit from a hot solution, or from a solution previously boiled and then cooled, was compared with that obtained from a cold, unboiled solution, the differences were irregular, the former being greater than the latter in amounts varying from 0.00–0.08 per cent. This was probably due to the expulsion of oxygen and CO_2 from the hot solution.

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[1] Zeitschr. für Instr., 8, p. 229, 1898.

It is possible that there might be a slight increase in deposit at higher temperatures which was not revealed, owing to some of the deposited silver having been dissolved by the hot solution. Kahle showed that hot water would slightly dissolve the deposited silver. And this was verified by the writer who found that the kathode with its coating of silver lost 0.2 mg. by being suspended during two hours in water maintained at 90° C. Now, it may be that at the instant of deposition the silver is more soluble than after it has been washed and dried. However, an increase of temperature probably does not of itself increase or diminish the deposit, as will more fully appear below.

Though the deposits from cold and hot solutions, respectively, did not differ in amount, they did differ in physical appearance. Those from cold solutions were always fine grained and snowy white, while those from hot solutions were more coarsely granular with a tendency towards gray in color.

In a further search for a temperature effect, the deposit from a boiled and then cooled water solution was compared with that obtained from fused silver nitrate. The results of the last seven experiments are here given:

Temperature of $AgNO_3$	**260°**	**255°**	**250°**	**245°**	**243°**	**242°**	**243°**
Deposit from solution,	0.7224	.7307	.90250	.79955	.8511	.88965	.9068
Deposit from fusion,	0.7230	.73075	.90285	.79955	.85115	.89080	.90665
Difference,	0.0006	.00005	.00035		.00005	.00115	.00015

Though these results show a difference in favor of the deposit from the fusion, yet this is probably accidental and due to some particles of foreign matter that were not washed off; for there was always more or less of a scum floating on the surface of the fused silver nitrate, some of which adhered to the kathode when removed and was difficult to dislodge completely.

In all the experiments of this paper, the water solutions employed were made by dissolving c. p. silver nitrate in distilled water in the ratio of 15 parts to 85. When fresh, these solutions were all neutral to sensitive litmus paper. After each experiment the solution was usually poured back into the bottle from which taken through a funnel and filter paper; subsequently it was taken directly from the bottle for use.

EFFECT OF "USED" SOLUTIONS.

Two different series of tests were made on the relative deposits from fresh and "used" solutions. The voltameters (Rayleigh form) consisted of two platinum dishes 7 cm. diameter, with round silver discs 4 cm. by 2 mm. The first "used" solution tested was made November 27th, its volume was 300 c.c., and from it about 52 grams of silver had been deposited when this series began, January 16th. Five tests were made and 69 grams of silver had been deposited from the solution when the last test began. The deposits in different tests varied in amount from 1.5 gr. to 2.26 gr. and the current from 0.77 amp. to 1.0 amp. The result of the five closely agreeing measurements gave an average deposit from the old solution greater than that from the fresh by 1 part in 11600.

More than two months later another series of tests on this point was made, the old solution in the meantime having been used a great many times, portions of it repeatedly boiled, and about 55 grams more of silver deposited from it. In this series the deposit from the old solution was compared with that from two different fresh solutions and was greater in amount by about 1 part in 1700. Moreover, this was a percentage increase as was proved by currents of different intensities and deposits of different amounts. But in every case in both these series of tests, the deposit from the fresh solution was more coarsely crystalline (varying from slightly to decidedly) and slightly less white in color than that from the old solution—just the opposite of what Kahle observed. Furthermore, a test at the conclusion of the work with litmus paper and with litmus solution failed to show, with certainty, any acidity in either the fresh or "used" solutions. This again is not in agreement with Kahle's observations. But Prof. Morse, of the Chemical Department of Johns Hopkins University, who was kind enough to make acidity tests for me, gave it as his opinion that the solutions were about neutral.

Possibly the boiling to which the old solution had been repeatedly subjected prevented it from becoming acid. This explanation is suggested by the fact that another solution, made some months later, became slightly acid on repeated use without boiling. This latter solution would turn sensitive litmus paper, but still it was not acid enough to prevent the permanent existence of a precipitate in 100 c.c. of the solution, due to the addition of $\frac{1}{10}$ c.c. of tenth normal sodium hydroxide. This solution, therefore, was decidedly less acid than Kahle's, but perhaps it had not been used so much, for only about 42 grams had been deposited from the whole volume amounting to 600 c.c. But the solution was five months old when the acidity tests were made. Age alone, however, did not render it acid for a solution equally old, but unused, was perfectly neutral.

The increase in deposit from "used" solutions must be due to some reduction of the valence of the silver in the electrolyte. Whether this reduction is due to the passage of a current, to light and age, to age alone, or to all combined is a question which it is proposed to submit to the test of experiment.

SIZE OF ANODES.

If, within ordinary limits, the relative size of anodes exercises any measurable influence it is important to establish this fact. It has been observed that larger deposits are often obtained in a large platinum dish than in a smaller one in series with the larger one. Schuster and Crossley expressed the opinion that this is due to the larger anode usually accompanying the larger dish.

To test this matter, the writer employed anodes of various sizes. At first two platinum dishes, each 7 cm. in diameter, were used, and the standard anode was a silver disc 4½ cms. by 3 mm. Two sizes of comparison anodes were tried one 3 cm. by 3 mm., and the other 1.8 cms. by 3 mm. The relative position of the anodes with respect to the battery was kept the same, but the position of the two bowls was alternated in each succeeding experiment. Thus the standard anode was alternately in each bowl, or dish.

Seven consecutive experiments with currents from 0.5–0.6 amp. and deposits of about 1.2 grams each led to the belief that there was no measurable effect due to the relative size of the anodes under the conditions of these tests. An unexpected result, however, appeared. In only one out of the seven tests were the deposits equal in amount. In the other six the deposit in one dish was larger than that in the other. But one dish was new and had a bright inner surface, whereas the other dish was old, its inner surface dull and scoured. In the six experiments there was deposited in the new dish a total of 7.1956 grams of silver and in the old one 7.19705 grams, which is 1 part in 5000 greater than the other. Now this difference could not be due to any relative error in the weights used to compare the deposits, for, in order to avoid this and obviate the necessity of making tedious corrections, the two dishes, by filing their edges, had previously been made very nearly equal in weight. Thus the bowls and their deposits were weighed with the same masses, respectively. These results seem to show, as Kahle has already observed, that the amount of the deposit is slightly dependent upon the nature of the surface receiving the deposit.

To test further for an anode effect, two platinum bowls, or dishes, each 9 cms. in diameter, the inner surfaces of which had been scoured, were used at kathodes, and the dimensions of the standard anode were 6 cms. by 2 mm. The comparison anode in the beginning was 1.8 cms. by 3 mm., but these dimensions of the small disc grew rapidly less, so that, at the end of the fourth test, the disc had been almost consumed, it weighing less than 1 gram.

The data follow :

No. test.	1	2	3	4
Amps.,	0.5	0.52	1.0	1.0
From large anode,	1.2303	1.0582	1.1592	1.1538
From small anode,	1.2304	1.0582	1.1591	1.1536
Difference,	—0.0001		0.0001	0.0002

These results seem to show that the relative size of the anodes, except possibly in extreme cases, has no appreciable influence.

Another point, tested by several experiments, was whether or not the filter paper wrapped about the anode in the Rayleigh form of the voltameter has any influence on the deposit. For this purpose a voltameter of the form previously used in the temperature experiments was connected in series with one of the Rayleigh form. J. H. Munktell's Swedish filter paper was used to wrap the anode.

It was found that the deposits were either the same in amount or their difference was within 0.2 mg.

The silver nitrate and anodes used in this work were obtained as c. p. from Eimer and Amend, New York.

The work was mostly done in the Physical Laboratory of the Johns Hopkins University, and the writer is indebted to the Directors of that Laboratory for valuable advice and assistance.

UNIVERSITY OF UTAH, SALT LAKE CITY.

A LECTURE DEMONSTRATION IN WAVE MOTION.

By Will C. Baker.

THE following is a description of a set of simple lecture demonstrations of the main phenomena of wave motion that have proved of great assistance to the writer in the past few years. As no description of them has been found in text-books or magazines, they are communicated in order that they may be of use to others. The experiments cover the same ground as the beautiful work of Vincent[1] and Wood,[2] but are adapted to lecture demonstration. The dimensions given are of small importance, but will serve to give an idea of the scale of the experiments.

A mercury tray—72 × 55 × 7 centimeters—whose smooth bottom is painted white, is filled to a depth of about half a centimeter with water, slightly blackened with ink. The proportion of ink to water will, of course, vary with the ink used, but the bottom of the tray must be clearly visible through the layer of fluid at rest. If now a beaker or other vessel of eight to ten centimeters diameter be placed in the tray a circular wave will be set up that will spread in all directions. The motion of this wave can be clearly followed as, the thickness of the fluid at the crest of the wave being much greater than at the trough, the appearance is that of a dark band travelling over a lighter background. The removal of the beaker gives a second wave; so, if the beaker be kept moving with a period of one or two seconds, a train of waves of period one-half to one second is produced.

It has been found best in lecturing to use either single waves or trains of not more than two or three, to avoid the interference effects after reflection at the sides.

Let a circular wave be set up as described. It travels across the tray at from fifteen to twenty-five centimeters per second and its reflection and the growth of the reflected wave at the sides of the tray can be easily followed on account of its slow motion. If a block of wood, say 50 × 3 centimeters by any thickness, be substituted for the beaker, a sufficiently good representation of a plane wave is obtained. Attempts have been made to eliminate the circular portions set up by the ends, by confining the disturbance between parallel guides, but the gain is so slight as not to warrant the loss in simplicity. With waves of this nature reflection of plane waves may be easily shown.

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[1] Phil. Mag., June, 1897; Feb., 1898; Sept., 1898; Oct., 1899.
[2] Phil. Mag., Aug., 1899.

A semicircular portion of barrel hoop placed at one end of the tray forms a reflector illustrating convex and concave mirrors. A plane wave sent into its concave side converges to a focus, or a circular wave set up in its focus changes after reflection to a plane wave in a manner beautiful to see. Circular waves after reflection proceed to the conjugate focus. These actions are best followed, especially by the class, when single waves are used.

Refraction phenomena are shown as follows : A piece of window glass, say 60 × 12 centimeters, is placed about one millimeter beneath the surface of the fluid and parallel thereto, care being taken that its surface is thoroughly wet. The waves will travel more slowly over this shoal part than they will over the deeper parts of their path. Circular waves in passing over this, decrease in curvature and have a shorter wave-length (for trains of waves) than over the deep part, and in passing off again increase the curvature and lengthen out, as described in the laws of refraction ; the displacement of the apparent center being clearly seen. Plane waves, as described above, at oblique incidences (say 60°) are refracted in passing onto the shoal, in a very marked manner.

A triangular piece of glass, of say 20 or 21 centimeter sides, with stops from its vertices to the sides of the tray to prevent disturbance from other parts of the wave front, enables us to show displacement of the image as with a prism ; the depth of the fluid over the glass regulating the retardation of the wave.

A circular or lenticular piece of glass, immersed at a suitable depth permits us to watch the changes in curvature of the wave front at the various surfaces and its final convergence to a focus as in a lens. As the best results are obtained with a comparatively small retardation a circular or nearly circular sheet of glass of say 20 centimeters diameter is used.

Huyghen's principle may also be illustrated by placing across the tray a stop having a gap of about one centimeter at its center. Any wave striking on one side of this is reflected except at the gap, which becomes the source of a circular wave proceeding into the smooth water beyond.

The diffraction or bending of waves around obstacles can be easily shown.

To obtain good results the tray bottom must be smooth, of a light color and must be parallel with the water surface. Single waves permit the class to follow each one through its changes, except, of course, where one is dealing with changes of wave-length when trains, must be used.

As waves, and not ripples, are used any lecture table is steady enough for the demonstration.

It may be necessary to draw attention to the fact that in all the foregoing we are dealing with waves passing over surfaces and not through media but the mechanism of the actions are similar, if not the same, in the two cases.

The claim is made that this method permits the class to obtain vivid ideas of the transformation of waves under the conditions dealt with, and that "retardation" and the various other terms applied to wave phenomena tend to take on clear meanings.

Physical Laboratory, Queen's University, Kingston, Ont.,
January 15, 1900.

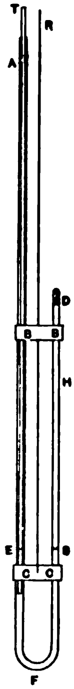
AN APPARATUS FOR THE DETERMINATION OF THE COEFFICIENT OF EXPANSION OF AIR AT CONSTANT PRESSURE.

By C. F. Adams.

THE determination of the coefficient of expansion of air is a problem eminently suited to the physical laboratory of the secondary school, but no apparatus that has hitherto been recommended for it is capable of giving accurate results even in expert hands, much less in the hands of a high school pupil. The practice of allowing pupils to use apparatus and methods that at best can give only very rough approximations to the truth is prejudicial to the cause of science and injurious to the pupil; and yet a great deal of the so-called quantitative work in our schools is of such a character. Much of the apparatus that has been prominently recommended for this work is so crude that in the hands of pupils unaided the results obtained are almost meaningless, and such apparatus placed in the hands of the instructor often requires an expenditure of time and effort highly impracticable and after all without any adequate return for the outlay.

The apparatus here described, it is believed, is not open to these objections. It is the result of a long series of experiments by Mr. N. H. Williams, of the Detroit Central High School, with a variety of devices in most of which he attempted to use mercury as a valve to enclose the air, and in which he failed to secure accurate results because of trouble with moisture.

AFH is a glass tube about 80 cm. long and 3 or 4 millimeters internal diameter. It is closed at *D* by a piece of glass rod fused in. The bend of the tube contains sulphuric acid to a depth of about 10 centimeters. *BB* and *CC* are strips of thin brass bent around the two legs of the tube. *CC* can be slid up and down by the wire *R*, while *BB* serves merely as a guide and support for the wire. *T* is a glass tube whose external diameter is slightly less than the internal diameter of *AFH* so that it will slide easily into the tube *A* but fitting so closely that it will remain at any desired position. This tube *T* serves as a plunger in the acid by which the surface of the acid at *E* can be adjusted to the same level as the acid at *S*.



The apparatus is first placed in a deep cylinder of cold water, a thermometer being supported beside it. By means of the wire *R* and the tube *T* the upper edge of *CC* and the acid at *E* are adjusted to the level of the acid at *S*. It is then taken out of the water and the distance from the edge of *CC* to *D* is measured by the mirror scale of a Jolly balance. It is then placed in warm water and the adjustments and measurement repeated. The apparatus has been used with temperatures between 10° and 50° C. Care must be taken of course to stir the water so that the temperature at top and bottom of the cylinder shall be the same. The coefficient of expansion is obtained by the formula

$$a = \frac{v_2 - v_1}{v_1 t_2 - v_2 t_1}.$$

Sulphuric acid is used instead of mercury because with the latter it was found impossible to avoid trouble with moisture. The vapor tension of the acid at the temperatures at which the apparatus has been used is so small that it can be disregarded. The apparatus is extremely simple and inexpensive and such that it can be manipulated successfully by high school pupils. The following are the results obtained by one of our laboratory sections: 0.00342, 0.00360, 0.00368, 0.00357, 0.00368, 0.00370, 0.00376. Another section reported the following results at the close of the laboratory session: 0.00363, 0.00364, 0.00369, 0.00368, 0.00364, 0.00371, 0.00351, 0.00366. Indeed the accuracy of the results is such that it would seem that in the hands of more advanced students the apparatus would give results in which the expansion of the glass could be taken into consideration.

DETROIT CENTRAL HIGH SCHOOL, December 19, 1899.

ON THE CHANGE IN LENGTH OF SOFT IRON IN AN ALTERNATING MAGNETIC FIELD.

By Louis W. Austin.

WHILE doing some work with one of my students on the change of length of iron due to magnetism, it occurred to me that there must be a similar effect in an alternating field, but perhaps modified in amount or character by the more or less rapid alternations. If the change in length requires an appreciable time for its completion, such modifications, it would seem must exist, and the probability of this seemed to me great enough to make it worth while to investigate the subject experimentally.

The instrument used in the investigation is based on an old principle long used in the lecture room to show qualitatively the expansion of solids due to heat. In its original form it consisted of a small roller placed under one end of a bar, which was held fast at the other end. When the bar was heated it expanded the roller rolled and by the movement of a pointer or small mirror attached to it showed to the class the expansion of the bar. In the improved form the horizontal roller is replaced by a vertical hanging fiber or fine wire which is held between vertical plates of plate glass about 2 cm. square, so that it is rolled when one of the plates is moved just as one might roll a thread between the thumb and finger. Below the plates a larger glass fiber of sufficient rigidity is attached to the rolling fiber and this bears a mirror and water damper. A second rolling fiber is hung between the plates at a little distance from the first in order to keep the surfaces apart to prevent friction. In the first form of instrument made and the one used in this work the fixed plate was cemented to a block of wood and the movable plate against which the iron rested was pressed against it by means of a rubber band. With this arrangement I have been able to get results with a glass fiber 0.01 mm. in diameter, but in general fibers as small as this are extremely difficult to adjust and are very apt to break between glass surfaces. I am now experimenting with other surfaces in the hope of getting better friction contact and less trouble in adjustment. I hope soon to be able to communicate a paper on this work.

Most of the observations recorded in the present paper were taken with a glass fiber about 0.06 mm. in diameter, though for a few of the later ones a fine wire was used. The latter observations being reduced to the scale of the former on the supposition that the maximum elongation with direct current was the same in both cases. Each of the observations given in the tables is the mean of from six to twelve observed throws and the good behavior of the instrument may be judged from the fact that in none of the series used, except in the case of frequencies greater than sixty per sec., did any of the single observations differ by more than two per cent. from the mean. The absolute calibration was made rather roughly by noting the distance the cross hair of the telescope appeared to move on a scale half a meter away when the moving plate was pushed forward by a fine micrometer screw.

The preliminary observations on the change of length in an alternating field were made on a soft iron bar 26 cm. long and 0.7 cm. in diameter, the same one with which I had been working with a direct field. The source of current was the city incandescent lighting circuit having a frequency of 120 per sec. by day and 60 per sec. by night. I found that the curves showing the relation between change of length and strength of field were similar in form for the alternating and direct currents, but that the maximum in the case of the alternating field fell some ten or fifteen per cent. below that produced by direct field, and that this decrease was greater with the frequency 120 than with 60 per sec.

These experiments were rendered difficult by the large heat expansion produced by the Foucault currents and hysteresis which of course made accurate observation somewhat uncertain. I have thus far found no remedy for the latter difficulty, but have rid myself of the former for the most part by replacing the solid iron bar by a bundle of wires of soft Swedish iron firmly bound together and insulated with varnish. The observations given in this paper were taken with such a bundle consisting of ten wires 27.5 cm. long and 1.4 mm. in diameter. The magnetizing solenoid was 24 cm. long with 12 turns per cm. The dilatometer with the fiber used (about 0.06 mm.) gave a magnification of about 127000 times with the scale 360 cm. distant. This gave a maximum reading of 14.5 cm. on the scale with direct field.

With apparatus kindly placed at my disposal by the department of electrical engineering[1] I was able to work with currents varying in frequency from 10 to 131 per sec. The values of H varied from 7.5 to 375, approximately, no correction being made for the influence of the ends of the solenoid. The larger value was a little more than enough to bring the iron back to its original length. With the stronger fields, especially with the higher frequencies, the heat due to hysteresis rendered the observations somewhat uncertain. I have been able, however, within the limits of field and frequency used, to get very consistent results by taking the readings by breaking the circuit and noting the distance the cross hair appeared to spring back on the scale. This motion is so sudden and so dead-beat that I do not think any larger error is introduced by the heat drift. Thinking that the water damper might influence readings taken in this way, several observations were taken without it, but no change in the throws could be detected.

In Table. I are given the changes in length expressed in ten-mil-

TABLE I.

H	Direct Current.	10 per sec.	30 per sec.	60 per sec.	120 per sec.
7.5	1.7	2.2	2.2		
15.0	9.0	13.2	13.2	14.4	13.2
30.0	29.5	32.4	29.0	25.8	24.5
45.0	38.5	34.5	35.0	31.0	29.4
75.0	41.5	39.8	36.6	34.1	31.0
150.0	30.1	28.0		27.0	
225.0	20.2	18.3			
300.0		8.0			
375.0	−2.8	−4.0?			

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[1] My special thanks are due to Mr. M. C. Beebe, instructor in electrical engineering, for his kindness in managing the electrical machinery used.

lionths of the length for direct current and for currents with alternations of 10, 30, 60 and 120 per sec., the strength of field varying from 7.5 to 375 C. G. S. The maximum comes at a value of *H* of about 75 in all the series, and the general course of the phenomenon is similar in all. With the lower values of *H* the elongations seem to increase more rapidly with the alternating currents than with the direct. This is perhaps due to the fact that the currents were read with an ammeter which indicated the square root of the mean square of current strength, which is very probably less than the effective strength of the alternating currents in this phenomenon. With all values of *H* above 15 the alternating current elongations fall below those with direct current. The curves of Fig. 1 show these relations graphically for direct current and frequencies of 60 and 120.

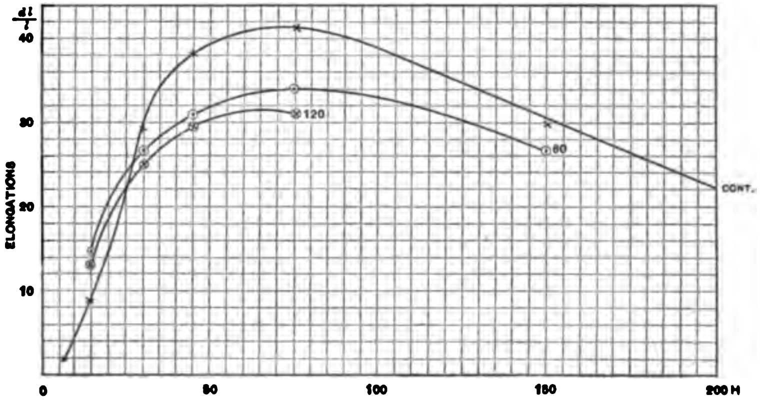

Fig. 1.

TABLE II.

Frequencies.	Maxim. expansion.	Frequencies.	Maxim. expansion.
Direct current	41.5	66 per sec.	33.2
10 per sec.	39.8	81	32.2
16	39.6	99	31.5
30	36.6	120	31.0
40	35.2	131	30.7
60	34.0		

Table II. gives the maximum elongation corresponding to different frequencies. A steady drop is noted as the frequencies become higher, which becomes less marked toward the end. Fig. 2, with its rather flat approximately exponential curve, shows the character of the change much better than the table can do.

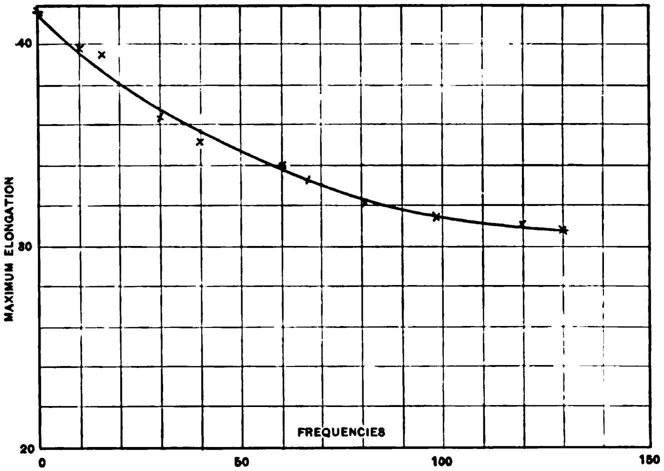

Fig. 2.

An attempt was made at one time to use the secondary currents of a transformer having a Wehnelt interrupter in the primary. The frequency as estimated from the pitch was about 800 per sec. The heating effects were so great, however, that no readings of value could be taken, the scale going by the eye far too rapidly to be read. The maximum certainly fell, much below any of the values given in the table and was estimated to be about half that found with the frequency of 131. The results of the investigation may be briefly stated as follows:

1. The general course of the change in length of soft iron in an alternating magnetic field is the same as in the direct field.

2. For all values of H above 25, and at least as high as 375, the length of the soft-iron specimen is less with an alternating than with a direct field.

3. The maximum elongation gradually becomes less as the frequency of the alternations is increased.

This continued decrease of the maximum elongation with increasing frequency is perhaps the most interesting fact brought out. It appears as though there were an appreciable viscosity[1] in the molecular movements depending on the rapidity of the alterations. The last two results may, it seems to me, have a connection with the observations of several physicists[2] on the decrease of permeability of iron when subjected to rapid magnetic reversals. The recent work of Niethammer[3] and of Wien[4] on this subject also shows a correspondence with the third result, Niethammer finding a percentage decrease in permeability with increasing frequency, agreeing fairly closely numerically with the results in this paper. Wien's percentage of decrease is considerably smaller. It can hardly be said, however, that the decrease in the maximum expansion is due merely to decreased induction, since if this were so it would seem that an increase of the field should raise the induction so as to produce the same maximum elongation as before, the only result being to make the maximum correspond to a slightly greater value of H. Since this is not the case the decreased maximum elongation cannot be considered a direct result of the decreased permeability, but their quantitatively similar decrease would indicate that they both may follow from the same molecular causes.

Exactly what course the elongation curves take with higher values of H, I have not been able to discover on account of the heating disturbances. The observations merely indicate that with the alternating current the iron is brought back to its initial length at a lower value of H than with the continuous current. Of the course of the phenomenon beyond this point I can say nothing, but I hope in the near future to overcome the difficulties which have restricted the scope of the work and to investigate the effects both of stronger fields and higher frequencies.

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[1] Since the completion of this work I have come across the observations of A. M. Mayer (Phil. Mag., 1873, Vol. 46, p. 177) on the time required for the full magnetic expansion to take place. These are the only observations made up to this time, so far as I am aware, that bear on the time factor in this phenomenon. He found that with his strongest current (25 Bunsen cells) $\frac{1}{10}$ second was required for the expansion and $\frac{3}{10}$ second for the corresponding retraction. While these estimates are certainly rough, they indicate the presence of the same appreciable viscosity which shows itself in the alternating field.

[2] Warburg and Hönig, Wied. Ann., Vol. 20, 1883, p. 814.

Tanakadate, Phil. Mag., Vol. 28, 1889, p. 207.

Klemenčič, Sitzungsber. d. k. Gesellsch. d. Wissench. zu Wien, Vol. 103, p. 17, 1894.

Weihe, Wied. Ann., Vol. 61, 1897, p. 578.

[3] Niethammer, Wied. Ann., Vol. 66, 1898, p. 29.

[4] M. Wien, Wied. Ann., Vol. 66, 1898, p. 859.

PHYSICAL LABORATORY OF THE UNIVERSITY OF WISCONSIN, January 10, 1900.

NEW BOOKS.

A Text-Book of Physics. By G. A. WENTWORTH and G. A. HILL. Pp. 440+vii. Ginn & Co., Boston. 1898.

The book before us is a disappointment. The authors have won an enviable reputation as writers of mathematical text-books, but in this book they show most convincingly that they are treading upon unfamiliar territory. True, there are many points about the book that deserve praise, and in the hands of a teacher who is a trained physicist the book may be useful; but to those who are only slightly acquainted with the subject, and these are the ones who will use the book most, it will be an unsafe guide. Errors for which it is difficult to find any excuse are unpleasantly frequent. A partial list of these has been prepared by the publishers, and is now sent with the book. Since this review must be brief a complete analysis of the book cannot be given, and the reviewer must content himself with giving a few of the reasons which have led him to the conviction that the book is an undesirable addition to our list of text-books.

On p. 19 we find the venerable fable that "Whatever the nature of the body or the kind of strain, the following simple law holds true: Up to the elastic limit strain varies directly as stress." That this may be branded on the memory of the pupil the same statement is repeated further on and three illustrative problems are given. But the fact is, as most physicists know, that this law of Hooke has been shown to be only a rough approximation.

In the chapter on heat among other minor errors is the statement, twice repeated on p. 102, that "the coefficient of expansion of all gases is equal to .00366" when even elementary books on the subject show that the coefficients for different gases lie all along the way from the value named up to about .00390.

In the discussion of the steam engine we find this statement regarding Carnot's principle: "A denial of its truth would lead to the absurd conclusion that energy can be created out of nothing." It leads to nothing of the sort. The law of the conservation of energy is no more affected by Carnot's principle than is the law of Moses, as was shown by Maxwell a generation ago. The authors state that our best engines "never have a greater efficiency than $\frac{1}{10}$." Ewing begs to differ.

In the treatment of electricity and magnetism the authors follow the beaten track of a generation ago, and the student finishes this portion of the subject without knowing that the medium surrounding a magnet or conductor plays any part. Such an important term as *strength of pole* is erroneously defined on p. 264 and a definition on p. 307 indicates that the international ohm has not yet been heard of. To Faraday alone is given the honor of having discovered induced currents—Henry is not mentioned. On p. 346 energy and power are confounded in the statement "Electric energy is the product of two factors, current strength and electromotive force." The shade of Ruhmkorff would be interested to learn that he and not Fizeau had "improved the induction coil by adding a condenser," but his feeling of pleasant surprise would give place to righteous indignation on being accused further on of having used in it "paraffined mica."

In judging any book one must know its aim. The aim of this book as stated in the preface is "to give a rational explanation of the more important physical phenomena, and to prepare the way for further investigation and study of physical science." If this was the aim at the start it seems to have been forgotten when the last division of the subject, light, was taken up. Reflection and refraction are treated without a word of reference to the wave theory; that is to say, these phenomena are *described*, not explained. Not only are explanations omitted but in many a place the facts are incorrectly stated. Sometimes the errors are typographical, at other times the authors are clearly at fault; for instance, in the article discussing the colors produced by thin films they say "According as this distance is equal to the wave-length or to half the wave length the rays will reinforce or destroy each other." Here they leave out of account the fact that there is at one point a change of phase of half a wave length, and of course, come to a conclusion exactly the reverse of the truth. Some of the optical diagrams are useless and that of the opera glass in particular is misleading and indefensible.

In determining how much space should be given to different subjects it is not to be expected that any two men will agree. Still an author who devotes no less than eight pages of his book to mirrors ought to have found space to mention the diffraction grating. A whole page is devoted to the bicycle while the important subject of thermal electricity has to be content with half a page. Half a page though seems to be more than the authors can write without drawing upon their imaginative faculties. They make the bismuth-antimony couple seven and a half times as powerful as it really is and further say that "for the same difference of temperature, bismuth and antimony give a stronger current than any other combination," a statement that needs some qualification in view of the fact that at least two other couples have much higher electromotive forces. In other cases that might be mentioned the authors make statements that are not sufficiently guarded.

One of the truly original ideas of the authors is expressed on p. 157, namely, that the diffusion of two gases illustrates adhesion; but in a text-book accuracy is preferable to originality of this sort. In conclusion the reviewer wishes to express his conviction that any man who writes such a text-book as this, so abounding in sins, both of commission and omission, is deserving of censure. To cast a stumbling block in the path of the young is no light offense.

JOSEPH O. THOMPSON.

AMHERST COLLEGE.

The Elements of Alternating Currents. By W. S. FRANKLIN and R. B. WILLIAMSON. 8vo, pp. 212. New York, The Macmillan Co., 1899.

This book is valuable, not only for what it contains but especially for what it omits, and its authors have succeeded admirably in presenting a clear outline of the essentials of alternating current theory and practice in a concise form, which may serve as a basis for a course of instruction in those subjects. It is so arranged that its treatment of the various divisions of the subject may be conveniently developed by the instructor in one direction or another as the circumstances under which the book is used may require.

Essentially a book for class instruction of electrical engineering students, it will, in an extended course, require amplification by the instructor, as obviously contemplated by the authors, for there has been no attempt to cover in the text many features of more or less importance. The fact that little else than the elements of the subject is included in the work serves to emphasize the fundamental conceptions in a particularly useful manner.

The book has many illustrative problems of a valuable character, together with their solutions, and at the close of most of the chapters are collections of problems for solution by the student which are well calculated to fix in the mind the principles of the chapters to which they are appended.

The first five chapters introduce the reader to the fundamental conceptions of monophase alternating current circuits and to the machines, instruments and measurements required for such circuits.

Chapters VI. and VII. present a satisfactory outline of the use of complex quantities and a number of illustrative problems which may serve as a basis for, and a valuable introduction to those methods which Mr. Steinmetz has so firmly incorporated in modern electrical engineering calculation.

Chapter VIII. introduces the conceptions of polyphase circuits and generators, while chapters IX., X. and XI. present in a more detailed manner the elements involved in the design of alternators and transformers.

The synchronous and induction motors and the rotary converter receive a satisfactory, though brief, treatment in chapters XII., XIII. and XIV., and in chapter XV. is given an exceedingly concise consideration of transmission lines.

The work as a whole is illustrated with well executed and pertinent diagrams, the typography is excellent, and the few errors in the types noted are to be expected in a first edition. The appearance of the book at the present time meets a distinct demand for a systematic outline on which a lecture or recitation course of instruction may proceed to any development required.

HAROLD B. SMITH.

De l'influence de la pression sur les actions chimiques. By GEORGES AIMÉ (1837), 12 pp.; with an introduction by P. DUHEM. 19 pp. Paris, A. Hermann, 1899.

The introduction is entitled *A point in the history of the Sciences. The tension of the dissociation before Sainte-Claire Deville.* Duhem has done a distinct service in bringing about the republication of this remarkable Thesis of Georges Aimé which, as he states, was brought to his notice by M. G. Brunel, Dean of the Faculty of Sciences at Bordeaux.

Duhem gives a biographical sketch of the author and a list of his publications. The remainder of the introduction is an interesting comparison of the work of Georges Aimé with the much later work of Sainte-Claire Deville and of Debray. The perusal, says Duhem, of the thesis of Aimé shows clearly that this physicist had reached the notion of the *tension of dissociation* in the form in which this notion grew out of the experiments of Debray thirty years later.

At the end of the eighteenth century Lavoisier had advanced the idea that pressure opposed the disaggregating force of heat so that at a given temperature a certain pressure would suffice to stop vaporization. Hutton in 1798 applied Lavoisier's idea to the explanation of the geological formation of marble by the melting of limestone without decomposition, the pressure due to superimposed strata being thought of as counteracting the tendency to decomposition. In 1804 the celebrated experiment of James Hall was communicated to the Royal Society of Edinburgh. This experiment was the melting of amorphous calcium carbonate under pressure and the formation of crystalline carbonate. In 1802 Dalton showed that the pressure which equilibrates the vaporizing tendency of a liquid is the pressure of its vapor only, a result which greatly extended the ideas which Lavoisier had previously advanced concerning the influence of pressure on vaporization.

What Dalton had done in extending the ideas of Lavoisier, Georges Aimé did in extending the theory of Hutton. He undertook to demonstrate that when a substance is decomposed by heat it is not the total pressure which is effective in arresting the decomposition, but only the pressure due to the particular gas which is generated by the decomposition.

Aimé showed in the first place that the decomposition of a carbonate by an acid in a closed vessel is arrested when the carbon dioxide formed reaches a certain pressure which depends upon the nature of the carbonate and of the acid. In the second place he showed that this pressure is independent of the proportions of carbonate and of acid. In the third place he showed that the arrest of the decomposition was determined by the pressure due to carbon dioxide only. He made no explicit statement concerning the dependence of this dissociation pressure upon temperature, although he must have considered the question, inasmuch as he was familiar with Hutton's theory and with Hall's experiment.

These results of Georges Aimé were entirely lost sight of, and it was in 1857 that Sainte-Claire Deville began again to approach the subjects Sainte-Claire Deville's experiments did not completely verify his hy. pothesis of a definite dissociation pressure. which hypothesis he advance- in 1863 when he had reached a clear notion of the matter, and it wad not until the researches of Debray were published in 1867 that this hypothesis became a matter of fact.

W. S. FRANKLIN.

Traité élémentaire de Mechanique chemique fondée sur la Thermodynamique. By P. DUHEM. Vol. IV., 381 pp. Paris, A. Hermann, 1899.

With this fourth volume Duhem's Treatise is completed in about fourteen hundred pages, constituting a most complete outline of the theory of the application of thermodynamics to chemistry. The first, second and third volumes of this Treatise have been previously noticed in the PHYSICAL REVIEW. This treatise represents an immense amount of labor on the part of the author and an exceptional breadth of knowledge of the subject.

It is characteristic of the application of thermodynamics to chemistry that it formulates the subject out of all proportion to our real knowledge, introducing unknown functions and a meaningless plurality of generalized coördinates. This monumental work of Duhem's is, therefore, of less use to the experimentalist than an equally laborious and erudite treatise might be in a subject lending itself less readily to the unbridled play of mathematical generalizations.

In this fourth volume the author discusses what he calls *les Melanges doubles* (double mixtures), meaning systems of two phases of variable composition, such, for example, as alcohol and water liquid and vapor.

W. S. FRANKLIN.

American Telephone Practice. By KEMPSTEAD B. MILLER. New York, American Electrician Co., 1899. Pp. x + 458.

In this volume Mr. Miller offers a wealth of detailed information concerning the systems of telephone construction in the United States. There is but little about the theory of the telephone, and that of the most elementary character, but the description of all the varied parts that go to make up the telephone exchange as it exists at the present day will be found of the greatest interest to all who care for the modern industrial development of electricity. One who is not already familiar with the subject cannot but be struck at the bewildering multiplicity of detail involved in the practical working of a device which when reduced to its essential features is so remarkably free from complicated mechanism as the telephone.

The opening chapters of Mr. Miller's volume lead the technical reader over very familiar ground. The description of the various forms of telephone receivers and transmitters, of batteries and of calling apparatus, together with the properties of circuits, are topics which have been treated in nearly every work on applied electricity which has hitherto appeared. Chapters 14 to 25, however, which form the bulk of the present volume, are entirely devoted to the means by which those who desire to communicate by telephone are brought together. In these chapters simple switchboards for small exchanges, listening and ringing apparatus for switchboards, self-restoring switchboard drops, complete switchboards for small exchanges, lamp signal switchboard, the multiple switchboard, transfer systems, common battery systems, house systems, etc., are treated in a very complete manner with diagrams of the wiring, and excellent illustrations of the more important pieces of apparatus. These follow a series of chapters on the various types of party lines, and the book concludes with a treatise on the selection of wire for telephone use, on the construction of pole lines, and on the laying of underground cables, together with a chapter on line and cable testing.

E. L. N.

Volume X. *April, 1900.* *Number 4.*

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THE
PHYSICAL REVIEW.

THE ACOUSTIC ANALYSIS OF THE VOWELS FROM THE PHONOGRAPHIC RECORD.

BY LOUIS BEVIER, JR.

THE use of the phonograph in the scientific study of vowel timbre is not new. It began more than twenty years ago when the instrument was still in an undeveloped stage, and much less true to timbre than it is today. One difficulty has been to secure a sufficiently magnified trace of the record for convenient study. The apparatus of Jenkin and Ewing[1] magnified ordinates 400 times, but was a machine of some complexity, and too delicate for general use by investigators. Its records, except in the hands of laboratory experts, would be untrustworthy.

Since that time many attempts have been made with more or less success, but I know of no apparatus so simple and inexpensive as that by means of which I have obtained, with the aid of my colleague, Dr. F. C. Van Dyck, Professor of Physics in Rutgers College, vowel-curves of very great clearness, and in which the ordinates are magnified about 1000 times. Moreover, the mere substitution of a different lens makes it possible to magnify as much more as may be desired, certainly up to 5000 times without much difficulty.

The apparatus used is illustrated diagrammatically in figure 1, page 194, which shows all the essential parts. We took an Edison automatic reproducer, such as is regularly sold with the machines, removed the diaphragm, and fastened to the tracing lever a rigid arm bearing an adjustable plane mirror. A spring, fastened to this arm, holds the sapphire knob of the reproducer lightly but firmly in the furrow of the wax. It should be just stiff enough to raise the weight of the movable plate, on which the lever is mounted,

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[1] Transactions of the Royal Society of Edinburgh, 1878.

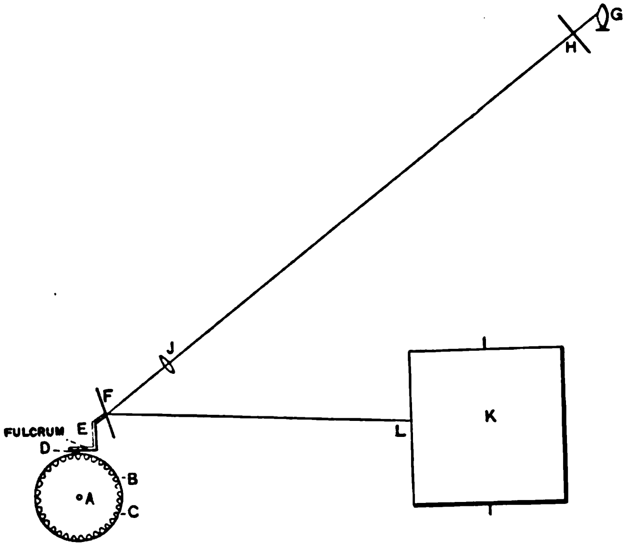


Fig. 1.

A = section of phonograph wax cylinder.
B = surface of the wax.
C = bottom of the furrow, with undulations much exaggerated.
D = sapphire knob of tracer.
E = rigid lever bearing adjustable mirror.
F = adjustable plane mirror.
G = source of light.
H = plate with pin hole through which light passes.
J = convex lens with conjugate foci at *H* and *L*.
K = revolving drum carrying bromide paper.
L = surface of paper on which point of light leaves the sinuous trace.

and press the whole snugly up against the carriage frame, in order that not the smallest movement of the knob can be taken up at bearings, but that every movement, small or great, shall be faithfully imparted to the mirror. Then a narrow beam of light reflected from the mirror and focused on moving bromide paper will leave a sinuous trace exactly corresponding to the profile of the bottom of the furrow in the wax. The magnifying power of this arrangement is very great, because the long arm of the lever is a beam of light, and making this arm longer is for the most part a very simple matter. To magnify 400 times by a system of material levers without falsifying the record by various accidental tremors was a matter of great difficulty, whereas by this plan it would be quite easy to magnify as much as 5000 times, if such enlargement should be found desirable. In my machine the distance from the sapphire knob to the fulcrum is about one-eighth of an inch, hence when the other arm is only ten feet we have magnified already nearly 1000 times. Thus far I have been content with this result, but have used a longer arm in a few experiments and found no new difficulties introduced. Making the short arm one-sixteenth of an inch, and working with a beam of light thirty feet in length would obviously magnify more than 5000 times. The chief difficulty would be to secure a fine point of light at so great a focal distance.

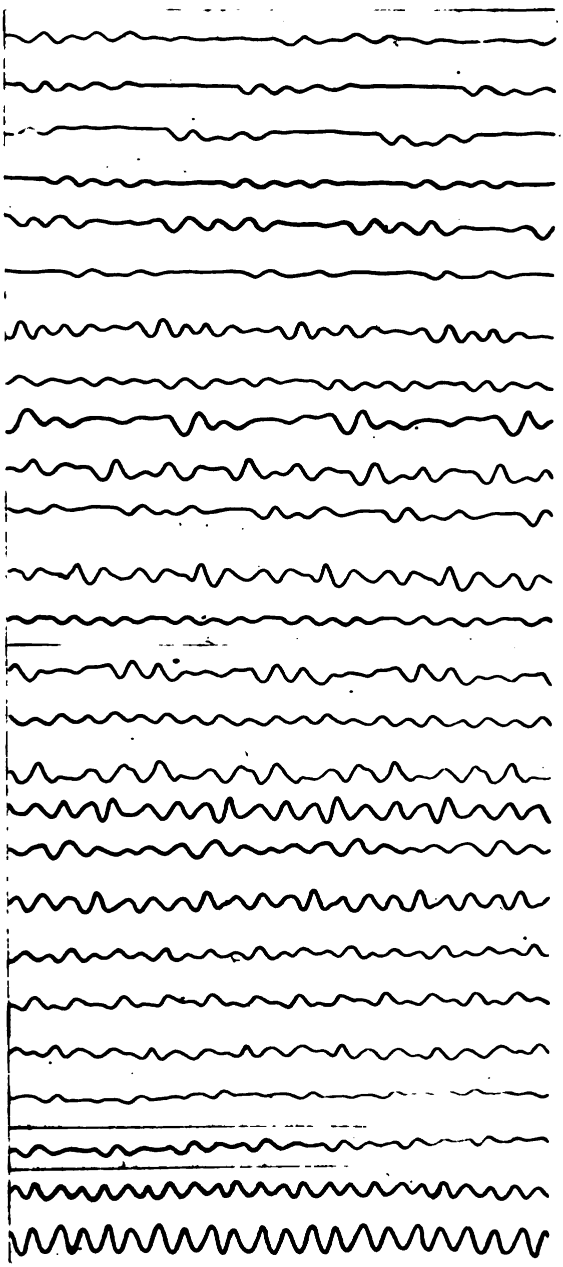
Besides this tracer one must have a good motor to turn the drum on which the bromide paper is carried. This should be adjustable, and as uniform in motion as possible, for if the drum does not revolve uniformly, the abscissas will be hopelessly falsified. Another motor also is needed which will give to the wax cylinder a very slow and uniform motion of rotation. When these preliminaries are arranged and the whole is installed in a room sufficiently dark for photographic work the difficulties of manipulation are not great. I may briefly describe the necessary steps. After the sound which it is desired to study has been sung into the instrument, and the sound-waves are thus recorded on the wax cylinder in the usual way, an ordinary reproducer should be placed in the carriage and a test be made to ascertain whether the instrument gives back the sound with truth and precision. One learns by a little practice to make records which come back with great perfectness of timbre, and this is most important if our results are to have permanent value. A record thus tested by reproduction is now ready to be transcribed. Remove the reproducer and insert in its place the transcriber already explained, adjusting carefully so that the knob runs precisely in the bottom of the furrow. Arrange the light so as to get as small and as perfect a point as possible on the drum. Place the bromide paper on the drum and start the two motors. The point of light will move up and down on the bromide paper, and as the drum revolves a sinuous line is recorded which becomes visible at once when the paper is treated with a developer. The drum should be so adjusted that as it revolves it will move either up or down on its axis so that the trace will not come round into itself. This may be done by hand, but it is better to have an automatic device such as can be arranged in any laboratory. On a strip of paper two feet long and six inches wide we may obtain a sinuous trace in this way of twelve feet or more, according to the amount of axial motion given to the drum with each revolution.

The actual depth of the hollows in the records carried by the wax cylinder is seldom more than 1/2000 of an inch, and generally very much less, so that when ordinates are magnified 1000 times the apparent amplitude as shown in the trace is seldom more and generally less than one-half an inch. An apparent amplitude of one half an inch makes a very loud sound in reproduction, and even when the greatest apparent amplitude is one-tenth of an inch the sound heard in reproduction is very clear and distinct.

With this apparatus I have transcribed many records of the principal vowels of our spoken English, but have as yet made a careful study only of the records of the vowel *a*, that is, the sound heard in such words as *father*, *palm*, *part*, etc. Plate I gives a photograph of some of the curves which I have obtained. They are all waves of the same sound at different pitches, as explained at the side. They were sung by four different voices, 1, 2 and 3 being baritones, and 4 being a boy's voice with soprano register. I have placed at the side the number of vibrations to the second as determined by the pitchpipe used in the present series of experiments. In photographing them they have been reduced nearly three diameters, so as not to occupy too much space. A few specimen waves of the full size as transcribed are shown in Plate II.

In transcribing the record from the wax cylinder to the bromide paper the cylinder was made to revolve once in about nine minutes, the bromide paper moving at the rate of 23½ inches (the circumference of the drum) in one minute. Now the circumference of the wax cylinder is about six inches, hence it is obvious that abscissas were magnified about 35 times (9 : 6 : : 1 : ⅔ ; ⅔ : 23½ : : 1 : 35¼). Again, since in making the original records the wax cylinder was made to revolve twice a second, it is obvious that a sound of one

CURVES OF THE VOWEL A.



Voice.	Pitch.	Number of waves.
2	113	2
2	144	2+
2	144	2+
2	152	3−
3	171	3+
2	171	3+
1	181	3+
2	202	3+
2	226	3+
1	226	4+
2	226	4+
4	240	4+
2	256	4+
2	256	4+
1	272	5
1	272	5−
4	272	5−
2	288	4+
4	288	5
1	304	5+
1	304	6−
4	320	6−
4	362	6+
4	384	7
4	426	8−
4	540	10+

BEVIER: ACOUSTIC ANALYSIS OF THE VOWELS.

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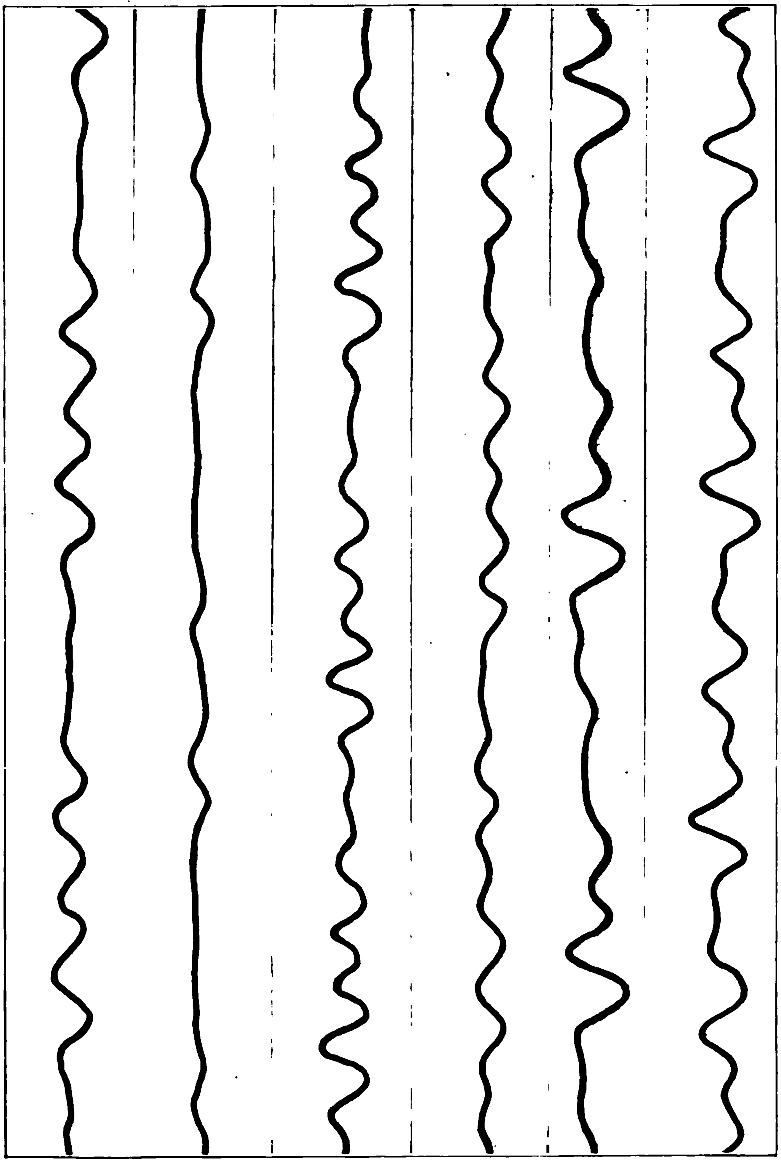

BEVIER: ACOUSTIC ANALYSIS OF THE VOWELS.

This plate reproduces in full size some of the curves shown in Plate I.

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second's duration would trace a sinuous line on the bromide paper about 35 feet in length. For a tone at the pitch d (288), the wave-length is therefore about 1½ inches, and at D (144) about 3 inches. In the plate the lines represent therefore about $\frac{1}{56}$ part of a second. These ratios were found to be in practice convenient, but provided that the pitch is in each case carefully registered there is no particular importance in preserving precisely the same relations. The fundamental waves are perfectly distinguishable by the eye alone, and if the pitch is known all the rest is a mere matter of computation. The slow movement of the wax cylinder in transcription is however very important, because we avoid in this way any tremors in the arm which bears the mirror, which otherwise would falsify the record. In the present series of experiments I have employed a speed less than one thousandth of the original velocity, and for some vowels of high resonance pitch (especially i, as in *pique*) I should be inclined to reduce the speed still more. Though the necessity for such slow motion makes experimentation somewhat laborious, since it requires 17 minutes to transcribe a sound of one second's duration, still the necessary preliminaries are so much more exacting of time that an hour more or less in actual transcription does not make much difference.

In one further detail my experience may be of use to other investigators. A cylinder which it is proposed to use in such studies must be carefully planed beforehand. Inequalities which are of no consequence in its ordinary use show as mountainous irregularities when the ordinates are magnified one thousand times. After a cylinder has been planed so that inequalities of this sort are negligible, the desired record should be made and transcribed with as little delay as possible, for the wax is quite sensitive to changes of temperature, and after being left over night is noticeably less regular than when first planed. So, too, after planing it must not be removed from the mandrel and replaced before being used, for it is impossible to replace it so exactly in its former position, but that it will when replaced show again more or less eccentricity.

The waves obtained, specimens of which are shown in Plates I. and II., were subjected to the usual Fourier analysis. In the longest waves, or waves of lowest pitch, 36 ordinates were measured, and the overtones up to the 16th were computed. For the shortest 12 measurements, giving the first four overtones, were found to be sufficient. Intermediate waves were measured in 20 or 24 places, according to their pitch. In no case were any overtones higher than the 10th of large enough amplitude to merit consideration. The subjoined table gives some specimen analyses running from A_1# (with 113 vibrations to the second according to the pitch-pipe used) to d′ (with 576 vibrations to the second). They represent four different voices, and establish with my other material, I believe, a fairly accurate acoustic definition of *a*. For a fuller exposition of these results I refer to an article soon to appear in "Die Neueren Sprachen."

	I.	II	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.
A_1#	113	226	339	452	565	678	791	904	1017	1130	1243
2	9.9	15.4	2.5	.8	**11.5**	**16.1**	3.3	8.1	**21.2**	7.9	3.2
D	144	288	432	576	720	864	1008	1152	1296	1440	1584
2	32.9	4.3	1.9	8.5	**14.8**	2.1	8.8	**13.5**	7.2	1.3	4.7
F	171	342	513	684	855	1026	1197	1368	1539	1710	1881
3	5.6	4.8	2.9	**17.5**	5.0	**33.1**	**19.9**	2.1	6.1	1.9	1.1
A#	226	452	678	904	1130	1356	1582	1808	2034	2260	2486
1	9.2	3.6	**34.6**	8.0	**31.0**	2.6	6.2	1.4	.8	1.2	1.4
d	288	576	864	1152	1440	1728	2016	2304			
1	5.4	**18.2**	8.0	**57.3**	3.0	1.3	3.2	3.6			
f#	362	724	1086	1448	1810	2172	2534	2896			
4	9.5	**14.3**	**42.9**	**16.5**	9.5	2.5	3.5	1.3			
a	426	852	1278	1704	2130						
4	5.7	**16.5**	**64.9**	6.0	6.9						
d′	576	1152	1728	2304	2880						
4	21.4	**68.0**	4.0	5.6	1.0						

N B. In this table the fundamental is marked I, the first overtone II, the second III, etc., following the common, but somewhat misleading practice. I have not thought it worth while to adopt Ellis's "partial tone" instead of the familiar "overtone."

It will be sufficient here to describe briefly the conclusions to which I have arrived. The vowel *a* at any pitch, and pronounced by any voice, contains the following elements:

1st, The fundamental, with the first two or three overtones, *i. e.*, the octave above the fundamental, the fifth, and the second octave.

The fundamental varies greatly in relative amplitude for reasons which I do not as yet attempt to formulate. The overtones are generally very weak, unless reinforced by the mouth resonance as set forth below.

2d, and most characteristic for the identity of the vowel; the overtone or overtones whose frequencies of vibration chance to fall between 1000 and 1300 vibrations to the second, the maximum seeming to lie at about 1150. This is the main resonance of the mouth when formed to utter this vowel, and remains remarkably constant no matter what the fundamental pitch may be. If the fundamental has 144 vibrations to the second, overtones VII., VIII. and IX., with frequencies of 1008, 1152, and 1296 respectively, will all be present, but VIII., with 1152 vibrations to the second, in much the largest amplitude. If the fundamental has 320 vibrations to the second overtones III. and IV. will be present, with 960 and 1280 vibrations to the second respectively, but IV. will be much the more prominent, III. being very weak since it falls below the region of strong resonance.

3d. The overtone or overtones whose frequencies of vibration chance to fall between 575 and 800 vibrations to the second for men's voices, with a maximum at about 675, or between 675 and 900 with a maximum at about 800, for the voices of women and children. This is presumably the resonance of mouth and throat cavities resounding as one vessel. It is not nearly as constant as the main resonance region explained above, and seems to vary between individuals and classes of voices far more than the other. It is however very important and at one pitch (A#, = 226) has an amplitude (not however an energy) a little greater even than the main resonance. If the fundamental has 144 vibrations to the second we shall find accordingly overtones IV. and V. present whose frequencies are 576 and 720 respectively, but V. being nearer the point of maximum resonance in this region will have the greater amplitude. At A# (= 226) we shall find overtone III., with 678 vibrations to the second very prominent. My analyses reveal for the vowel *a* no other region of resonance except these two.

I subjoin a tentative chart (Fig. 2) representing the resonance curve for the *a*-position of the man's mouth. In this I have made the frequency of vibration the abscissa and the amplitude of vibration the ordinate. Were this based on a long series of observations to eliminate individual and accidental elements we could thus plot a curve which could be taken as the standard resonance curve characteristic of the vowel. It would be practically an objective definition of the vowel, deviations from which in individual cases would be such as would not affect materially the character of the vowel.

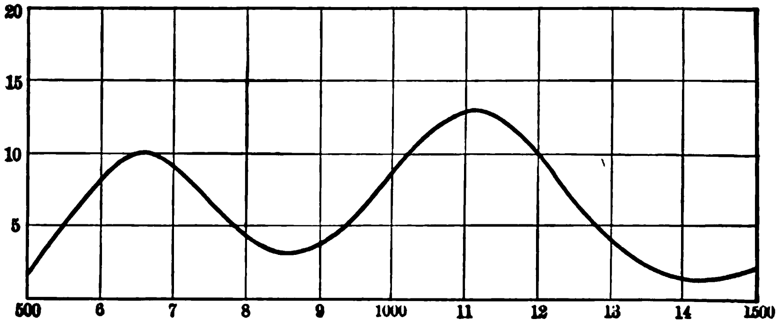

Fig. 2.

The above curve (Fig. 2) is drawn to represent simply amplitude percentages. The "energy" curve, in which ordinates are proportionate to the product of the squares of amplitudes and the squares of frequencies (E varries as a^2n^2) is perhaps even more significant. A tentative energy curve follows :

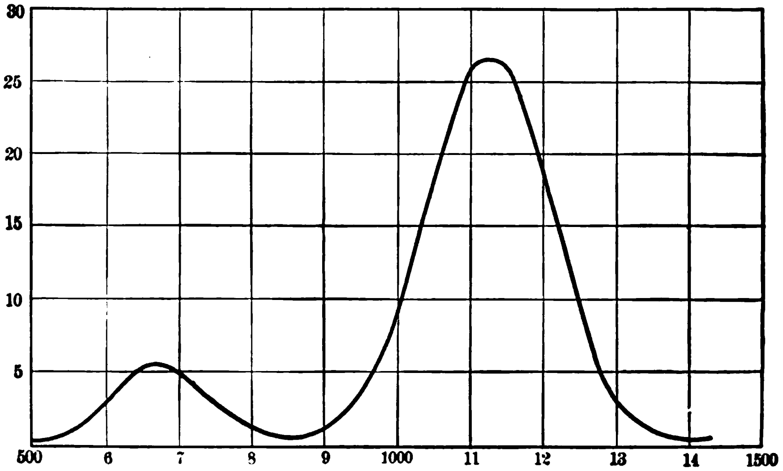

Fig. 3.

Many important questions arise which I do not as yet attempt to answer. A preliminary discussion of some of them will be found in the article to which I have already alluded. My main purpose here is to call attention to a means of experimentation which I am convinced will prove of great use in further researches.

A word as to the mathematical analysis by means of Fourier's theorem may be helpful. It is well to use the average ordinates obtained from a succession of adjacent waves. I say adjacent waves because no matter how steady the voice has been the waves are apt to change slightly in character as the sound proceeds, and though adjacent waves are often, so far as the eye can detect, perfectly similar, it is rarely possible to obtain a sequence of more than a dozen or so which are to the eye exactly alike. Changes of pitch, timbre or intensity so slight as to escape the notice of the ear cause very noticeable modifications in the wave-form, and make it illegitimate to average the measurements of any save adjacent waves. As the succession of waves is examined it will generally be seen that a change is in progress which, though slow, results in the course of a small fraction of a second even in quite a marked difference. An analysis at two different points of the same utterance. gives quite a different succession of measurements for ordinates, but the results of a mathematical computation are very similar. Before this matter can be really settled it will be necessary to analyze each wave of a series from beginning to end of an individual utterance. A sufficient number of such computations will probably throw light on this very interesting and difficult subject.

After the necessary measurements have been obtained the analysis is made by means of the familiar formulæ derived from Fourier's theorem, but the labor, particularly when it is necessary to use as many as 36 ordinates, is by no means small. I am indebted to my colleague, Robert W. Prentiss, M.S., Professor of Mathematics in Rutgers College, for a method of arranging the formulæ which has greatly abridged the labor. He is preparing an explanation of his arrangement which will be published shortly.

NOTE ON THE VARIATION OF THE SPECIFIC HEAT OF WATER BETWEEN 0° AND 100°C.

By H. L. Callendar and H. T. Barnes.

THE present note which it is thought would be of interest to many of the readers of the Physical Review is, with a few changes made to introduce later results, almost a reprint of the report communicated by the authors at the meeting of the British Association at Dover (Sept., 1899). In 1897, during the Toronto meeting, a preliminary note was communicated to the same society, in which the authors described briefly a new method for determining the specific heat of a liquid in terms of the international electrical units. At that time the method had only been roughly applied to the cases of water and mercury. The preliminary apparatus, however, was exhibited to several members of Section A on the occasion of their visit to McGill University. One of the main objects of the work was the determination of the variation of the specific heat of water over the range 0° to 100°C. for which the method is peculiarly suited. Shortly after the commencement of the work one of the authors (H. L. C.) was called to London to fill the Quain Chair of Physics in University College, leaving the work in the sole charge of H. T. B.

The general principle of the method, and the construction of the apparatus will be readily understood by reference to the diagram of the continuous-flow electric calorimeter given in Fig. 1. A steady current of water flowing through a fine tube, *AB*, is heated by a steady electric current in a central conductor of platinum. The steady difference of temperature between the inflowing and outflowing water is observed by means of a differential pair of platinum thermometers at either end. The bulbs of these thermometers are surrounded by thick copper tubes, which by their conductivity serve to equalize the temperature, and to prevent the generation of heat by the current in the neighborhood of the bulbs of the thermometers. The leads *CC* serve for the introduction of the current, and the leads *PP*, which are carefully insulated, serve for the measurement of the difference of potential on the central conductor. The flow tube is constructed of glass, and is sealed at either end, at some distance beyond the bulbs of the thermometers, into a glass vacuum jacket, the function of which is to diminish as much as possible the external heat loss. The whole is enclosed in an external copper jacket (not shown), containing water in rapid circulation at a constant temperature maintained by means of a very delicate electric regulator.

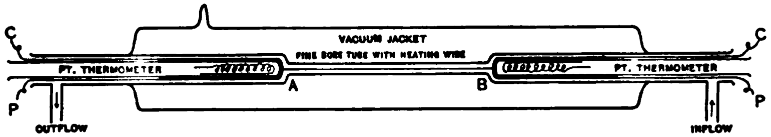

Fig. 1.

The general equation of the method may be stated in the following form:

$$JMd\theta + H = ECt.$$

The difference of potential *E* on the conductor is measured in terms of the Clark cell by means of a Thomson-Varley slide potentiometer, very accurately constructed and calibrated. The current *C* is also measured by observing the difference of potential on a standardized resistance *R* included in the circuit.

The Clark cells employed in this work were "crystal" cells[1] of the B. O. T. pattern and hermetically sealed cells. They were kept immersed in a regulated water bath at a constant temperature, near 15° C., and have maintained their relative differences constant to a few parts in 100,000 since they were made in 1895.

The resistance *R* consists of two one-ohm coils in parallel, each made of four bare platinum-silver wires in parallel wound on mica frames and immersed in paraffin oil at a constant temperature. The coils were annealed at a red heat after winding on the mica frames, and are not appreciably heated by the passage of the electric currents employed in the work. The two one ohms are repeatedly standardized by comparison with an oil immersed certified one ohm manganin standard of the English Electrical Standards Committee, and have not changed their resistance since being made by as much as 5 parts in 100,000.

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[1] H. L. Callendar and H. T. Barnes, Proc. Roy. Soc., 62, 117 (1897).

The time of flow t of the mass of water, M, was generally about fifteen minutes, and was recorded automatically on an electric chronograph reading to .01 second, on which the seconds were marked by a standardized clock.

The letter J stands for the number of joules in one calorie at a temperature which is the mean of the range $d\theta$, through which the water is heated.

The mass of water, M, was generally from 400 to 600 grammes. After passing through a cooler, it was collected and weighed in a tared flask in such a manner as to obviate all possible loss by evaporation.

The range of temperature was generally about 8° in the series of experiments on the variation of J, but other ranges from 1° to 12° were tried to test the theory of the method. The thermometers were read to the ten-thousandth part of a degree, and the difference was probably in all cases accurate to .001° C. This order of accuracy could not have been possible with mercury thermometers under the conditions of the experiments.

The external loss of heat, H, was very small, owing to the perfection and constancy of the vacuum attainable in the sealed glass jacket. It was determined and eliminated by adjusting the electric current so as to secure the same rise of temperature, $d\theta$, for widely different values of the water-flow.

The great value of the continuous-flow method as compared with the more common method in which a constant mass of water at a uniform temperature is heated in a calorimeter, the temperature of which is changing continuously, is that in this method there is practically no change of temperature in any part of the apparatus during the experiment. There is no correction required for the thermal capacity of the calorimeter; the external heat loss is more regular and certain, and there is no error due to the lag of the thermometers. Another distinct advantage is that the steadiness of the conditions permits the attainment of the highest degree of accuracy in the instrumental readings.

In work of this nature it is recognized as being of the utmost importance to be able to detect and eliminate constant errors by varying the conditions through as wide a range as possible. In addition to varying the electric current, the water-flow, and the range of temperature, it is possible with comparatively little trouble to alter the form and resistance of the central heating conductor, and to change the glass calorimeter for one with a different degree of vacuum, or a different bore for the flow tube. Several different calorimeters were thus employed, and the agreement of the results, on reduction, affords a very satisfactory test of the accuracy of the method.

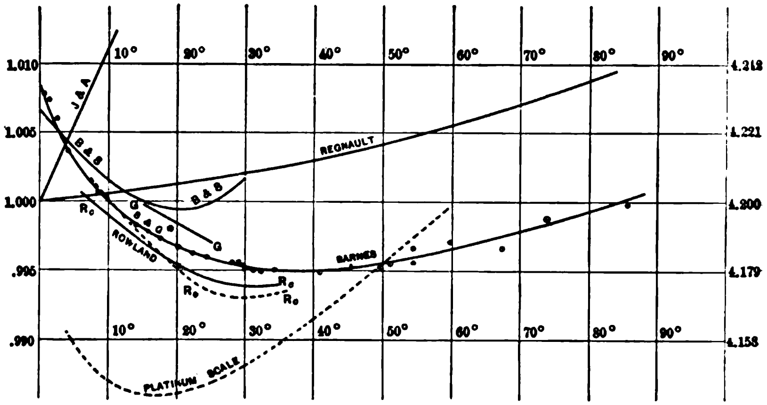

Fig. 2.

Variation of the Specific Heat of Water between 0° and 100° C.

J. & A. Jamin and Amaury, 1870.
B. & S. Bartoli and Stracciati, 1891.
$R_\bullet$. Rowland's Original Values, 1880.
R_c. Rowland's Corrected Values, 1898.
G. Griffiths, 1893.
S. & G. Shuster and Gannon, 1893.
⊙ Authors' Values, 1899.
} Lord Rayleigh's Value for the Clark Cell, Assumed.

* Authors' at 20.°2 C. } Kahle's and Carhart's Identical Clark Cell Values, Assumed.

The general results of the investigation, so far as it is possible to give them for publication at present, may be seen by reference to the plot given in Fig. 2, which includes also the results of other observers, to the same scale. The curve marked Regnault represents the well-known formula of Regnault which has been adopted as the basis of much calorimetric work. This formula was confessedly approximate, and was deduced from experiments on mixing water at high temperatures with water at 15° C. The method could not be expected to give any information with regard to the variation of the specific heat at ordinary temperatures. The experiments of Jamin and Amaury (J. and A.), by the method of electric heating, gave a very rapid increase of the specific heat at low temperatures, but the science of electrical measurement, and the difficulties of the electrical method, were not at that date sufficiently known to render the results of any value.

Rowland's original curve (R_o) is shown by the dotted line, and with it his corrected values[1] (R_c) are given by the smooth line. His original results, obtained by Joule's method, were expressed in terms of his own air thermometer, and showed, for the first time, how rapidly the specific heat of water diminishes as the temperature rises from 0° to 30° C. The corrected values are those obtained by reducing his thermometers to the Paris scale. The effect of this correction is to lower the temperature at which the specific heat is 4.200 joules from 10° to 7° C. and to diminish the temperature coefficient. In the plot in Fig. 2 the right hand margin gives the scale of joules in absolute measure.

The experiments of Bartoli and Stracciati (B. and S.) were made by the method of mixtures, and are expressed in terms of a thermal unit at 15° C. Their curve bears a general resemblance to that of Rowland, but shows a minimum point at 20° C. The errors and limitations of this method are well known, and it is difficult to suppose it capable of any high degree of accuracy, or to think otherwise than that the excessive lowering of the minimum point is due to some error not eradicated by mere repetition of similar experiments.

The experiments of Griffiths (G.) between 15° and 25° were made by observing the rate of rise of temperature of a mass of water heated by an electric current. His work threw a flood of light on the difficulties of electric calorimetry, and explained the failure of previous observers to obtain satisfactory results by this method. Over the range of his experiments he found approximately the same rate of diminution of the specific heat as that given by Rowland.

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[1] William S. Day, Physical Review, 7, 193 (1898).

The curve marked by circled dots in the figure, extending from 0° to 86° C. represents the results so far obtained in the present investigation. The points represented are samples of single observations with different calorimeters, and give an idea of the order of accuracy attainable by this method. The accuracy of course diminishes as the temperature rises, owing to the greater difficulty of obtaining satisfactory regulation in the water-jacket at the higher points.

The curve shows clearly that the minimum point occurs at 40° instead of 30°. The experiments of Rowland did not extend sufficiently beyond his minimum point to obtain a satisfactory determination. The value of the specific heat could not be determined by his method with the same accuracy at the extremities of the range as in the middle, and all the probable errors of the method would be greatly increased as the temperature of the calorimeter was raised above its surroundings. In particular, the corrections and changes of zero of the mercury thermometers, and the rate of external heat loss, would be excessive at the higher points. In the present work, however, there are no thermometric difficulties of this nature, owing to the use of platinum thermometers, and the external heat loss increases very little as the temperature is raised, because the external water-jacket is always at the same temperature as the inflowing water, so that the mean excess of temperature is always nearly the same. Another indication that the temperature of minimum specific heat is not far below the middle of the range is given by the experiments of Regnault, and more recently by those of Reynolds and Moorby, on the mean specific of water between 0° and 100° C. Their results by entirely different methods agree in showing that the mean specific heat over the whole range does not greatly exceed the value at 20° C.

The present work shows that the specific heat of water approaches an exceedingly high value at 0°. The observations at 0.6°, 1.5°, 2° and 4° were obtained by using rises of temperature of

1°, 3°, 4° and 8° with the inflow water nearly at 0°. This rapid increase, shown by these points, was foreseen by Rowland, but not shown in his work.

It is interesting to compare the absolute values of the specific heat deduced by the electrical method with those of Rowland by the mechanical method. For this purpose the authors' results, and those of Griffiths (G.), and of Shuster and Gannon (S. and G.), have been reduced to joules on the assumption that the absolute value of the E. M. F. of the Clark cell is that found by Glazebrook and Skinner, assuming Lord Rayleigh's value of the electro-chemical equivalent of silver, and taking the international ohm as correct. It has been pointed out that the results of Griffiths would be brought into harmony with those of Rowland by supposing that the true E. M. F. of the Clark cells employed was about 2 millivolts lower, or one part in 700. The authors' results, however, lie about midway between those of Rowland and Griffiths, and would require a correction of only 1 millivolt, if the whole of the difference were to be ascribed to the Clark cell. It is not at all likely that the cells used in this work can have exceeded the B. O. T. standard by so much as 1 millivolt, or that the resistance standards can have been incorrect by as much as one part in 700. It is most likely that both the Clark cells and the resistance standards agreed with those used by Griffiths to within one or two parts in 10,000 and that the difference of the results is mainly to be attributed to the radical difference in the methods of calorimetry. In view of the recent determinations of the absolute value of the Clark cell made by Kahle and later by Carhart and Guthe, the authors' value at 20° C. is reduced assuming the new value of the Clark cell, *i. e.*, 1.4330 volts. This has the effect of lowering the specific heat and causing it to almost exactly coincide with Rowland's corrected value at the same temperature. The complete variation curve would then be lowered the same amount had the new Clark cell value been used in its deduction.

These and similar questions relating to the absolute values of the standards used do not affect the accuracy of the relative results as regards the variation of the specific heat of water with temperature. The relative results are regarded by the authors as being as accurate as the present apparatus is capable of affording. By far the most important consideration affecting the form of the curve is the particular thermometric scale to which the results are reduced. If, for instance, the results were expressed in terms of the platinum scale, which differs from the absolute scale by only 0.38° C. at 50° C., when the divergence is a maximum, the curve would be that represented by the dotted line in the figure. The authors' variation curve is deduced from this by the usual parabolic formula, which gives results in practical agreement with the Paris scale.

Since the discussion of the thermal unit introduced by Griffiths at the British Association meeting of 1895, and partly in consequence of the general interest excited by that discussion, so many new facts have been brought out, and so much experience has been gained of the practical effect of the proposals then made, that it appears desirable to discuss more fully the bearing of the present work on the general question of the relation between the various thermal units.

Dieterici[1] made a determination of the mean specific heat in terms of the electrical units by means of a Bunsen ice-calorimeter. His result gives 4.233 joules as the value of the mean specific heat of water in absolute measure between 0° and 100° C.

Winkelmann[2] endeavored to connect this result with Rowland's at low temperatures by assuming a parabolic formula for the mode of variation, and taking the minimum value at 30.6° C. to be 0.9898 of the value at 0° C. This formula makes the ratio of the mean specific heat between 0° and 100° C. to the specific heat at 20° C. to be 1.0120. According to Regnault's formula the same ratio would be 1.0038. If we take Rowland's corrected value at 20° C. as 4.181 joules, the mean value between 0° and 100° would be 4.197 joules according to Regnault, but 4.233 joules according to Winkelmann. The latter gives a remarkable coincidence with Dieterici, in consequence of which the formula of Winkelmann has been frequently quoted and employed in physical investigation. It must be remarked, however, that Rowland's curve is not even approximately parabolic and that the range covered by his observations is hardly sufficient to justify this method of treatment. It must also be observed that the values given by Winkelmann's formula for the specific heat in the neighborhood of 100°, and still more at higher temperatures, are so large that they cannot possibly be reconciled with the experiments of Regnault and other good observers.

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[1] Wied. Ann., 33, 417 (1888).

[2] Handbook of Physics, Vol. II., Pt. 2, p. 338.

Griffiths[1] came to the conclusion from a comparison of his experiments on the latent heat of evaporation of water at 30° C. and 40° C. with those of Dieterici at 0° C. expressed in terms of the mean specific heat, and with those of Regnault on the total heat or steam at 100° C. that the mean specific heat must be very nearly identical with the specific heat at 15° C., although Regnault's direct experiments made the ratio from 0.5 per cent. to 1 per cent. larger. At his suggestion Prof. Joly performed the inverse experiment of determining the mean specific heat between 12° and 100° with his steam calorimeter in terms of the latent heat of steam at 100° taken as 536.63 times the thermal unit at 15° C. The result of this experiment was to make the mean specific heat appear nearly 0.5 per cent. smaller than the specific heat at 15° C. If we suppose that the inversion of the experiment would tend to reverse the error of the original determination of the latent heat, the result would appear to be strongly in support of Griffith's contention.

Peabody, in the preface of his "Tables of the Properties of Saturated Steam" (1896), as a result of a careful discussion of Rowland's and Regnault's experiments, adopts Rowland's values from 0° to 40°, and expresses his results in terms of the mean specific heat between 15° and 20°. He finds that Regnault's experiments may be sufficiently represented in terms of this unit by assuming the specific heat to be constant and equal to 1.008 between the limits 45° and 155°, and constant and equal to 1.046 between the limits 155° and 200° C. This assumption would make the mean specific heat between 0° and 100° have the value 1.0044 in terms of the specific heat at 17.5° C., or the value 1.0056 in terms of the specific heat at 20° C., assuming Rowland's coefficient of diminution. The general effect of these changes is to make the tables agree fairly well throughout with Regnault's experiments, but the method can only be justified on the ground of expediency, and can hardly be regarded as a satisfactory reconciliation of conflicting evidence on account of the assumed discontinuities in the specific heat.

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[1] Phil. Trans., 7, 318 (1895).

Shaw[1] gives a similar reduction of Regnault's experiments by means of Rowland's original table, but tabulates only the total heat in joules at each point between 100° and 180° C. His reduction shows a similar flattening of the curve between 100° and 150°, as compared with Regnault's formula. This may be a physical fact, but might also be explained by supposing that the earlier experiments at 108° to 120° were about 0.4 per cent. too high. Shaw's reduction expressed in terms of a thermal unit at 20° C. is given for comparison in the table.

Recently a direct determination of the mean specific heat in terms of the mechanical units has been made by Reynolds and Moorby on a large scale with Reynolds' break and a steam engine. Their results expressed in absolute measure is 4.1832 joules, and is entitled to very great weight on account of the minute accuracy of the measurements, and the full discussion of possible sources of error. It exceeds the value found by Rowland at 20°C. by only one part in two thousand, but is no less than 1.20 per cent. smaller than the mean value found by Dieterici. This is a discrepancy far too large to be explained by any uncertainty in the values of the electrical units.

Accepting the trustworthy measurements of Reynolds and Moorby it is clear that the minima of specific heat at 20° and 30° indicated by the work of Bartoli and Stracciati and of Rowland respectively, must be due to some source of error in their methods, and that all formulæ hitherto proposed for the mode of variation of the specific heat between 0° and 100° must be abandoned.

It is possible, however, to deduce a more satisfactory comparison of the results of Rowland with those of Reynolds and Moorby by means of the present series of experiments, on account of their great range, and the close agreement of the individual observations. Neglecting for the present the rapid change of the specific heat in the immediate neighborhood of 0° C., it may be observed that all the authors' observations between 10° and 60° (with the exception of one at 55°) are represented to about one part in 5,000 (*i. e.*, within the limits of agreement of the observations with different calorimeters at any one point) in terms of the minimum value S_{40} at 40° C., by the simple formula

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[1] B. A. Report, 1896, p. 162.

$$S_t = S_{40}(\ 1 + .0000045(t - 40°)^2\ ).$$

which gives for the mean specific heat between 0° and $t°$ the formula

$$S_0^t = S_{40}(1.0072 - .00018t + .0000015t^2).$$

If this formula could be assumed to hold beyond these limits over the whole range 0° to 100, the ratio of the mean specific heat between 0° and 100° to the specific heat at 20° would be 1.0024. Assuming Rowland's 4.181 joules at 20°, this ratio would give the value 4.191 joules for the mean specific heat, a result obviously in excess of Reynolds' and Moorby's 4.183 joules.

It is evident from the observations at higher temperatures that the variation curve is not symmetrical, but flatter between 60° and 100°. The rate of change of the specific heat at 100° as given by the above formula, if extrapolated, is more than twice as great as that given by Regnault, and at 200° about four times as great. The experiments of Regnault apply particularly to this portion of the range, for which they have remained the standard, and have been so universally adopted. It would be desirable to retain his formula for the present, with such change as would make it fit the present experiments. It will be readily seen by reference to the figure that Regnault's curve is very nearly parallel to the authors' curve between 40° C. and 86° C. The two curves can then be very accurately fitted over this range by simply subtracting a constant quantity from the values given by Regnault's formula. This method leads to results, which require the simplest modification of existing tables.

Adopting the authors' formula between 10° and 60°, and the corrected formula of Regnault from 60° to 100°, the ratio of the mean specific heat between 0° and 100° to the specific heat at 20° is 1.0014. Taking Rowland's value as 4.181 joules at 20°, this ratio would give 4.1868 for the mean specific heat, which exceeds the value found by Reynolds and Moorby by less than one part in a thousand—a discrepancy so small as to be within the limits of

TABLE OF THE SPECIFIC HEAT OF WATER.

RANGE 0° TO 60°C.

FORMULA.

$$S_t = 0.9982 + 0.0000045\,(t - 40°)^2.$$

t°C.	Joules.	S_t	$S_{40}{}^t$	k Authors' Formula.	Rowland (reduced).
0	4.203	1.0054	—	—	—
5	4.196	1.0037	1.0045	5.023	5.023
10	4.190	1.0022	1.0037	10.037	10.044
15	4.185	1.0010	1.0030	15.045	15.054
20	4.181	1.0000	1.0024	20.048	20.057
25	4.178	0.9992	1.0018	25.045	25.053
30	4.176	0.9987	1.0013	30.039	30.043
35	4.174	0.9983	1.0009	35.032	35.039
40	4.174	0.9982	1.0006	40.024	Peabody
45	4.174	0.9983	1.0003	45.016	45.000
50	4.176	0.9987	1.0001	50.008	50.040
55	4.178	0.9992	1.0000	55.002	55.080
60	4.181	1.0000	1.0000	60.000	60.120

Range 60° to 220°C. Regnault (Corrected).

Formula $S_t = 0.9944 + 0.00004t + 0.0000009t^2$.

60	4.181	1.0000	1.0000	60.000	60.12	
65	4.184	1.0008	1.0000	65.002	65.16	
70	4.188	1.0016	1.0001	70.008	70.20	
75	4.191	1.0024	1.0002	75.018	75.24	
80	4.195	1.0033	1.0004	80.032	80.28	
85	4.199	1.0043	1.0006	85.051	85.32	
90	4.203	1.0053	1.0008	90.075	90.36	
95	4.207	1.0063	1.0011	95.105	95.40	
100	4.212	1.0074	1.0014	100.138	100.44	
110	4.222	1.0097	1.0020	110.22	110.67	Shaw.
120	4.232	1.0121	1.0028	120.33	120.73	
130	4.243	1.0148	1.0036	130.47	130.80	
140	4.255	1.0176	1.0045	140.63	140.88	
150	4.267	1.0206	1.0055	150.82	151.01	
160	4.281	1.0238	1.0066	161.05	161.20	
170	4.295	1.0272	1.0077	171.31	171.61	
180	4.310	1.0308	1.0089	181.60	182.14	
190	4.326	1.0345	1.0102	191.94	—	
200	4.342	1.0384	1.0115	202.31	—	
210	4.359	1.0425	1.0130	212.72	—	
220	4.377	1.0467	1.0145	223.18	—	

possible error even in the case of these two extremely accurate determinations.

The annexed table has been constructed on the basis of the above considerations and represents, by the authors' formula combined with the corrected formula of Regnault, the variation of the specific heat of water between 0° and 220° C. The absolute measure at 20° C. given by Rowland, *i. e.*, 4.181 joules, is taken as the basis of column 2. The values of the specific heat are worked out assuming a unit at 20° C. and neglecting the rapid increase below 10° C. The general effect of this table is to diminish the extent of the variation hitherto assumed, but it is believed that the results here tabulated are within the limits of error of all the best determinations. The order of agreement may be inferred from a comparison of the values of h, the total heat of the liquid, given in the last two columns. The agreement with Rowland is within 1 in 3,000, between 10° and 40°, and with Regnault within 1 in 1,000 at 160° C. The variations of Regnault's individual observations exceed 5 parts in 1,000.

The values of the total heat h are obtained by integrating the specific heat from 0° to t° by the formula. By adding below 10° the constant quantity .020 to the value of h as given in the table, account is taken of the rapid increase at the freezing point.

In conclusion it may be stated that the work is being rapidly continued to verify the points obtained above 60° C. The observations at the higher points are exceedingly difficult and tedious to obtain but with the present arrangements designed especially with this object in view observations as high as 95° C., will probably be obtained.

MACDONALD PHYSICAL LABORATORY, MCGILL UNIVERSITY, Feb. 3, 1900.

THE MAGNETIC MOLECULAR SUSCEPTIBILITY OF SALTS OF THE IRON GROUP.

By O. Liebknecht and A. P. Wills.[1]

GUSTAV WIEDEMANN was the first to undertake magneto-chemical investigations in regard to the behavior of the paramagnetic salts of the metals: chromium, manganese, iron, cobalt, nickel and copper. He used a torsion method with a non-uniform field.[2] He found that the quantity defined by him as the "Molecularmagnetismus" is nearly independent of the solvent, the concentration, and especially of the nature of the anions for a given temperature and cation of a given valency. The differences always exceeded the experimental errors and amount in general to 2 or 3 per cent. Therefore he found a mean value for a given cation and called it the "Atommagnetismus." The atomic magnetisms of the manganous, ferrous, cobaltus and nickelous salts were as $a + 2b$, $a + \frac{8}{2} b$, $a + b$, a, where $b = 1.15a$; that of the ferric salts exceeds that for the ferrous salts by about 15 per cent.

Not long since G. Jäger and St. Meyer[3] published some striking measurements made with a modification of the Quincke manometric method. They drew the conclusion that the atomic magnetisms of manganese, iron, cobalt, nickel are as 6:5:4:2; this agrees with the proportion given above if $a = b$. Their results were obtained from the chlorides, sulphates and nitrates; with manganese, cobalt and nickel, only the bivalent salts could be used; on the other hand the value for iron was taken as the mean of the results for ferric chloride, ferrous sulphate and ferric nitrate.

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[1] The chief results were communicated to the Physikalische Gesellschaft on the 30th of June, 1899; many results were still outstanding.

[2] G. Wiedemann, Pogg. Ann., 126, p. 1, 1865; 135, p. 177, 1868; Wied. Ann., 5, p. 45, 1878; P. Plessner, Wied. Ann., 39, p. 336, 1890; G. Quincke, Wied. Ann., 24, p. 392, 1885. G. Wiedemann, Lehre von der Elektricität, 2. Aufl. 3, §§ 1266–1291, 1895. J. Koenigsberger, Wied. Ann., 66, p. 698, 1898.

[3] G. Jäger und St. Meyer, Wien. Ber. (2), 106, pp. 594, 623, 1897; 107, p. 5, 1898.

In later articles Jäger and St. Meyer use for ferrous chloride the relative number 3 instead of 5 as above; further, they obtain from chromic chloride and chromic sulphate the number 2.5 for chromium.

In the light of these results it seemed to us desirable to make new determinations using a different method. We have made quantitative determinations upon 36 salts of the five metals of the iron group and of copper. Great stress was laid upon the procuring of the purest material possible, and all salts in which there was danger of rapid decomposition were discarded. In general, we have studied the sufficiently soluble fluorides, chlorides, bromides, iodides, sulphates and nitrates; for the reason just mentioned, it was not possible to study many of the iron and chromium compounds. The accuracy of our final results may vary between 2 and 5 parts in 1,000, according to the method used in the chemical analysis.

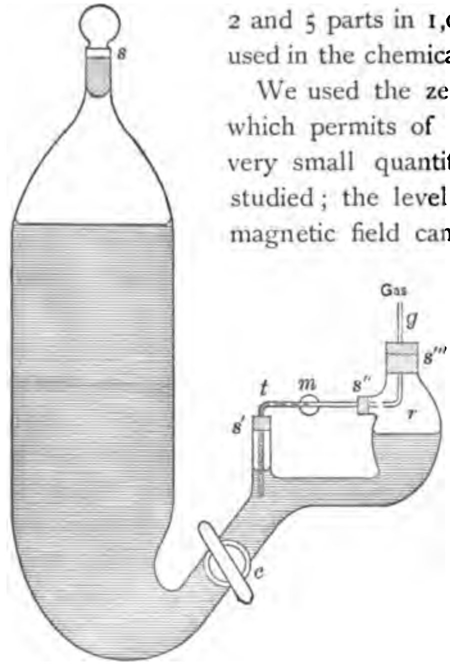

Fig. 1. ¼ natural size.

We used the zero method of Prof. du Bois,[1] which permits of successful manipulation with very small quantities of the substances to be studied; the level of the capillary tube in the magnetic field can be so adjusted that the unmagnetic solution can be prepared with great precision so as to be well within the limits of error in the subsequent chemical analysis. Briefly, the procedure is as follows: A conveniently strong paramagnetic solution is prepared and then diluted with the diamagnetic solvent until the solution is unmagnetic. The practicability of the method is only limited by the susceptibility and solubility of the salt. Sometimes the method is inadmissible on this account as, for example, in the case of potassium chromate and bichromate and of potassium and calcium permanganate. These peculiarly constituted salts are well known to be but weakly paramagnetic; the similarly constituted dry K_2FeO_4 proved to be much more paramagnetic; the alkali solution decomposed so quickly that we were prevented from making a quantitative determination.

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[1] H. du Bois, Wied. Ann., 35, p. 154, 1888; 65, p. 38, 1898.

As a criterion for the unmagnetic condition of the solution, the behavior of its meniscus in a Quet's[1] capillary tube *t* in a magnetic field was observed (Fig. 1). The large mixing vessel had a capacity of about 600 ccm.; it could be closed by means of a glass stopper *s*, and could be separated from the rest of the apparatus by means of the glass stopcock *c*; the arm connected with the mixing vessel was so formed that fresh capillary tubes could be suitably introduced and fixed by means of the rubber stoppers at s' and s''. The space beyond the meniscus in the capillary tube and the space *r* could be filled with washed moist illuminating gas.[2] This apparatus was fixed to a suitable stand permitting a rotation about a horizontal axis perpendicular to the plane of the figure. The inclination of the capillary tube could be thereby regulated by means of a suitable adjusting screw; and, moreover, by means of a vigorous rotation of the apparatus about this axis a thorough mixing of the solution could be attained. At *m* is represented the end view of the truncated conical pole piece of the ring electro-magnet.

With a little practice a very short time sufficed to prepare an unmagnetic solution with an accuracy of one part in 1,000, so that upon excitation of the electro-magnet no noticeable motion of the meniscus took place. At each dilution the mixing apparatus was, of course, well shaken and each part well washed with the diluted solution, so that the solution should have in all parts the same concentration as at the meniscus. Any possible local change in the concentration due to magnetic influences would scarcely have time to be effected; furthermore, though such an effect theoretically exists, it has not been unmistakably shown experimentally.[1] In case the capillary is inclined toward the horizontal by a fraction of a degree, it is possible with a field of 40,000 c. g. s. units, to detect a differential susceptibility which does not exceed a hundred thousandth of that of water. On account of the above mentioned limits of error in the chemical analysis, it was useless to try for such accuracy as this, so that as a rule a much less sensitive adjustment was employed.

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[1] Quet, Comp. Rend., 38, p. 562, 1854; E. Verdet, Oeuvres, 1, p. 199

[2] The susceptibility of illuminating gas was neglected; as far as known it amounts at a pressure of one atmosphere to less than one thousandth of that of water; compare M. Faraday, Exp. Res., 3, p. 502, 1855; G. Quincke, Wied. Ann., 34, p. 445, 1888; A. Toepler u. R. Hennig, Wied. Ann., 34, p. 796, 1888. An absolutely unmagnetic gas can be made after the manner of an unmagnetic solution by mixing a little oxygen with a diamagnetic gas; compare du Bois, l. c., p. 167; of course it is here tacitly supposed that the permeability of a vacuum serves as the unit in the usual conventional way.

The procuring and cleaning of the capillary tubes offered at first some difficulties. Finally the freshest possible tubes from Thüringer glass were used, these being kept under water until required.[2] With these the mobility of the meniscus was satisfactory. Any influence of the breadth of the capillary tube used upon the concentration of the unmagnetic solution contained therein, could not be detected. This furnished, as is easy to see, an indirect proof that the capillary constants of unmagnetic solutions are quite unaltered in the magnetic field.

This independence of the surface tension of magnetic influences seems to obtain in general.

Brunner and Mousson[3] have fruitlessly sought such an action, whereby they seem to have overlooked the well known-Quincke effect of elevation (or depression) in the magnetic field, which, however, W. Wertheim[4] probably observed. Quincke[5] first undertook drop experiments with strong paramagnetic manganese and iron solutions; he found that the weight of a drop was unaltered by a uniform magnetic field and assumed, therefore, the constancy of the surface tension of such solutions. On the other hand, in a non-uniform field between conical pole pieces the drops of manganese chloride as of iron chloride were larger or smaller upon the excitation of the magnetism according as they formed under or above the shortest lines of force.

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[1] Compare with G. Wiedemann, Electricität, 2, Aufl. 3, § 1205, 1895.

[2] Compare P. Volkmann, Wied. Ann., 66, p. 194, 1898.

[3] C. Brunner u. A. Mousson, Pogg. Ann., 79, p. 141, 1850.

[4] W. Wertheim, Comp. rend., 44, p. 1022, 1857; Pogg. Ann., 102, p. 595, 1857.

[5] G. Quincke, Pogg. Ann., 160, p. 586, 1877; Wied. Ann., 24, p. 375, 1885. Compare also Durchflussbeobachtungen by H. Dufour, Lum. Electr., 23, p. 337, 1887; likewise G. Jäger u. St. Meyer, Wied. Ann., 67, p. 711, 1889.

For a further investigation of these phenomena we constructed an apparatus (Fig. 2) by means of which the formation of the drop could take place at the nozzle *n* which was of about 1 mm. inside breadth, and at the proper pressure and at different rates, and moreover either in a uniform field of about 18,000 c. g. s. units between surface pole pieces, or in a non-uniform field of about 40,000 c. g. s. units between conical pole pieces *M* and *M'* and while the nozzle was placed in the position of greatest field intensity. The vessel *A* was in connection through *S* with the reservoir *B* in which the level of the liquid could be regulated. The space *r, r* could be connected through the cock *c* with a pump; the pressure regulated in this manner was read off on the manometer *m*. A number of experiments were carried out with this apparatus and, briefly stated, the results are as follows:

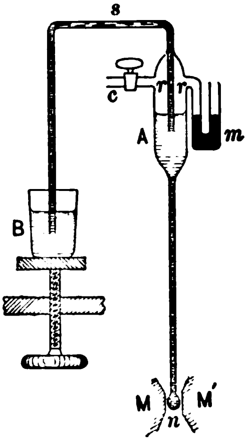

Fig 2. ⅙ natural size.

Paramagnetic solutions show in a uniform field, an increased rate of dropping when the magnetic force upon the liquid column acts downwards; through diminution of the pressure the original rate being again approximately established,[1] the weight of the drop suffers no change. On the other hand, we observe an enlargement of the drop in a non-uniform field; the form of the drop is then changed and depends upon the particular distribution of the field as regards the sense and manner in which it happens. These experiments confirm Quincke's results and conclusions.

Diamagnetic water shows in a uniform field a slower rate of dropping when the magnetic force is directed upwards and, therefore, against the hydrostatic pressure; under proper conditions the dropping may be quite stopped through excitation of the field. If the original rate of dropping be restored through increase of the pressure the weight of the drop suffers no change. In a non-uniform field the weight appeared less with the arrangement used, and more so the less uniform the field.

[1] **This must be done because, as well known, the rate under ordinary conditions exerts a small secondary influence upon the weight of the drop.**

Unmagnetic solutions are not at all influenced, as regards the magnitude of the drop, through excitation of the field if the drops fall in an atmosphere of illuminating gas. Toward air they behave, naturally, like a weakly diamagnetic liquid. This simple criterion has an application in the preparation of unmagnetic solutions; in accuracy this procedure is inferior to the method already described and the one, as a rule, used by us.

THEORY OF THE METHOD.

The extension of our knowledge of magnetic phenomena might be expected, from the first, in a thorough investigation of the thermo-magnetic and magneto-chemical processes. It would seem then to be not superfluous to give in this place, concisely as possible, the elementary conceptions connected with calculations on this subject. The reference of the magnetic moment to the unit of mass, once much employed, has been quite displaced in consequence of the advanced development of the mathematical theory of electromagnetism, because in mathematical physics the reference to the unit of volume is simpler. Professor du Bois [1] has shown that in some cases it is desirable to return to the original method, because the results are in many respects more easily comparable when the variable density is eliminated. Since magnetic processes are essentially molecular it is still better to refer them for stoichiometrical purposes to molecular masses.

In what follows m means molecular weight; M, mass; $\mathfrak{M}$, magnetic moment; $\mathfrak{S}$, specific magnetization, *i. e.*, moment per unit of mass; $\mathfrak{H}$, field intensity; i, specific susceptibility defined by the relation $i = \frac{\mathfrak{S}}{\mathfrak{H}}$.[2] The molecular susceptibility i_m is now defined by the equation

$$i_m = mi = \frac{m\mathfrak{S}}{\mathfrak{H}}.$$

In what follows, the subscripts l, w, s refer respectively to the solution, solvent and salt. On account of the additive properties, first shown by G. Wiedermann, of the quantities here coming into consideration, the following equation obtains for a solution in a uniform field

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[1] H du Bois, Phil. Mag. (5), 29, p. 293, 1890.

[2] If we designate, as customary, the density with D, the magnetization with $\mathfrak{J}$, the susceptibility according to the ordinary definition with κ, then is $\mathfrak{S} = \frac{\mathfrak{J}}{D}$ and $i = \frac{\kappa}{D}$. Compare J. Koenigsberger, Wied. Ann., 66, p. 698, 1888.

$$\mathfrak{M}_l = \mathfrak{M}_w + \mathfrak{M}_s.$$

The influence of form as well as the therewith connected magnetic interaction of the separate similar or dissimilar volume elements may be with the strongest paramagnetic solutions disregarded since the square of the susceptibility in comparison with unity is quite negligible. In particular for an unmagnetic solution $\mathfrak{M}_l = 0$ therefore $\mathfrak{M}_s = -\mathfrak{M}_w$, or $\mathfrak{S}_s M_s = -\mathfrak{S}_w M_w$. After division by $\mathfrak{H}$ we get

$$(1) \qquad \frac{i_s}{i_w} = -\frac{M_w}{M_s}$$

and the relative molecular susceptibility $\frac{i_m}{i_w}$ of the salt becomes finally

$$(2) \qquad \frac{i_m}{i_w} = -m\frac{M_w}{M_s},$$

i. e., equal to the negative product of the molecular weight into the mass ratio of the solvent and salt. This value of i_m holds not only for the particular concentration of the unmagnetic solution but quite generally;[1] whether, considering the accuracy of our new method, it is quite independent of the kind of solvent can only be decided by a special series of experiments; it changes markedly with the temperature.

Professor du Bois has shown that the behavior of an unmagnetic solution in fields of different intensities permits a conclusion as to the constancy of the susceptibility and has shown this constancy to exist with fields up to 10,000 c. g. s. units.

If the constitution of the unmagnetic solution measured by $\frac{M_w}{M_s}$, is independent of the field intensity, then it follows directly from equation (1) that the relative susceptibility with reference to water $\frac{i_s}{i_w}$ is also independent of the field.

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[1] According to the results of various investigators; compare with J. Koenigsberger, l. c., p. 708.

[2] H. du Bois. Wied. Ann., 35, p. 155, 1888.

In fact, such a dependency was not found for any of the unmagnetic solutions investigated, and this holds for the extended range of fields between 2,000 and 40,000 c. g. s. units. Now it can be asserted with a probability bordering upon certainty that the variations of the susceptibility of water would not be followed exactly proportionately by those of the different salts. Therefore, the constancy of the absolute susceptibility of the dissolved salt, as of water, within the specified range of fields, follows from the constancy of the susceptibility of the dissolved salt relative to water.

Furthermore, it is very improbable that the susceptibility for weaker fields would be variable.[1] In the results to follow the constant susceptibility of water enters everywhere as a principal factor. We have the following determinations for this factor which have been recalculated so as to refer to a temperature of 18°, and to a vacuum; the susceptibility of air at one atmosphere pressure and 18° was taken as 0.023×10^{-6}.[2]

TABLE I.

	Temperature 18°.	Toward Vacuum.
1885	G. Quincke (most accurate value)	$\kappa = -0.797 \times 10^{-6}$
1888	H. du Bois	-0.831 "
1892	S. Henrichsen	-0.727 "
1895	P. Curie (mean value)	-0.788 "
1896	J. S. Townsend	-0.735 "
1898	J. Koenigsberger	-0.785 "
1899	G. Jäger–St. Meyer (mean value)	-0.645 "
	Mean value	$\kappa = -0.758 \times 10^{-6}$

Since $D_{18^\circ} = 0.9987$, i_w becomes -0.759×10^{-6}

In consideration of the obvious uncertainty of the value of the fundamental constant i_w and its temperature coefficient we have for the present designated it by x and taken the molecular susceptibility relative to it; and then in order to obtain approximate absolute values we have taken provisionally $x = -0.75 . 10^{-6}$. A thorough discussion of the above values and further accurate new determinations would be desirable. So soon as these can be carried through with the accuracy with which the relative values $\frac{i_m}{x}$ are determined, the reduction of the latter to absolute numbers may be easily accomplished.

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[1] Compare Vgl. F. Auerbach in Winkelmann's Handbuch der Physik, 3, 2 Abth., p. 217. Breslau, 1895.

[2] G. Quincke, Wied. Ann., 24, p. 404, 1885; H. du Bois, Wied. Ann., 35, p. 167, 1888; S. Henrichsen, Wied. Ann., 45, p. 53, 1892; P. Curie, Thèse Nr. 840, p. 31, Paris, 1895; J. S. Townsend, Proc. Roy. Soc., 60, p. 186, 1896; Beibl. 20, p. 796, 1896; J. Koenigsberger, Wied. Ann., 66, p. 703, 1898; G. Jäger und St. Meyer, Wied. Ann., 67, pp. 427 u. 707, 1899. The old determinations of M. Faraday, E. Becquerel, J. Schuhmeister and the value of Théodore Wahner's which falls so far out are not considered.

The constitution of an unmagnetic solution is variable with the temperature; we have therefore carefully maintained a normal room temperature of 18°C.[1] The unmagnetic solutions prepared according to this are then at higher (lower) temperatures somewhat diamagnetic (paramagnetic) because the temperature coefficient for the salt is always greater than for the water.

In case the latter were accurately known, the former for different salts could easily be determined.[2]

The Chemistry Details.

Most of the salts were pure preparations from Kahlbaum or Merck, and were, when possible, many times recrystallized before use. Most of the fluorides, bromides and iodides were prepared by one of us from pure carbonates. The nickel salts were procured from Kahlbaum as "cobalt-free" and the cobalt salts as "nickel-free"; the influence of the chemically unrecognizable impurities present cannot make a difference in the molecular susceptibility fonnd of more than 0.5-per-cent. solutions, which especially incline to decomposition, *e. g.*, MnF, MnCl, FeSo, FeCl, FeBr, were usually treated with a free acid; this was without noticeable influence on the results since the susceptibility of the acid added differs little from that of water; the ferrous iodide contained some free iodine. Where a gradual decomposition was to be feared, the solution was analyzed as soon as prepared; quickly decomposable solutions were not investigated, as already remarked; of course, care was taken that no dilution or concentration change should take place after the solution was once prepared. The gravimetric analysis was carried out for the chromium salts by precipitating them with ammonia as hydroxides. The manganese solutions were precipitated with phosphorus-free hydrogen peroxide and ammonia as oxides and weighed as mangano-manganic oxides. The iron salts were determined in part with potassium permanganate by titration and in part precipitated with ammonia as hydroxides. Finally, the cobalt, nickel and copper were electrolytically precipitated, the two first in a neutral solution with ammonium oxalate and the copper in a solution of sulphuric acid containing a little nitric acid.

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[1] Compare F. Kohlrausch und L. Holborn, Leitvermögen der Elektrolyte, Leipzig, 1898.

[2] In such measurements, an unmagnetic atmosphere should bound the liquid. The marked dependence of the susceptibility of oxygen relative to air upon the temperature, which has already been mentioned, would otherwise bring in a considerable error.

Results.

Our quantitative results are collected into Table II. q designates the valency of the cation of the salt; a the atomic weight; the metals are arranged according to their increasing atomic weights.[1] The position of cobalt and nickel in the series is reversed, however, after mature consideration. For, according to the collective properties in the metallic or amalgamated condition, and in the condition as a paramagnetic compound, cobalt stands nearer to iron than does nickel; likewise for the chemical properties. In the column before the last is given the relative molecular susceptibility x, which is calculated from the equation given above. In the last column the absolute value is given under the provisional assumption $x = -0.75 \times 10^{-6}$ which number may be a number of per cent. false.

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[1] The atomic weights are from those collected by the German Chemical Society (Chem. Ber., 31, p. 2761, 1898; Wied. Beibl., 23, pp. 69 und 315, 1899, whereby $O = 16.00$). The question as to the atomic weight of cobalt and nickel is still a subject of chemical discussion; compare Cl. Winkler, Zeitsch. anorg. Chem., 17, p. 236, 1898; Th. W. Richards and G. P. Baxter, l. c., 21, 251 and 22, p. 221, 1899; St. Meyer, Wied. Ann., 69, p. 263, 1899, hints at the possibility of an inverted order in the series.

[2] Recalculated according to the collection of W. Ostwald, Stoichiometrie, 2. Aufl., p. 512, Leipsig, 1891.

TABLE II.

Temperature 18°C. $x = -0.75 \times 10^{-6}$

	Name of Salt in Water Solution.	q	Formula.	O — 16.00		$\frac{M_w}{M_s}$	$-\frac{i_m}{x}$	$+i_m$
				a	m			
1	Chromium Sulphate.	3	$Cr_2(SO_4)_3$	52,1	392,38	40,73	2×7990	0,00599
2	Chromium Potass. Alum.	3	$CrK(SO_4)_2$	52,1	283,37	29,63	8400	0,00629
3	Chromium Amm. Alum.	3	$CrNH_4(SO_4)_2$	52,1	262,30	30,77	8070	0,00605
4	Chromium Nitrate.	3	$Cr(NO_3)_3$	52,1	238,22	35,28	8400	0,00629
5	Manganous Fluoride.	2	MnF_2	55,0	93,00	215,71	20060	0,01504
6	Manganous Chloride.	2	$MnCl_2$	55,0	125,90	163,10	20530	0,01540
7	Manganous Bromide.	2	$MnBr_2$	55,0	214.92	94,63	20340	0,01524
8	Manganous Iodide.	2	MnI_2	55,0	308,70	65,49	20220	0,01516
9	Manganous Sulphate.	2	$MnSO_4$	55,0	151,06	133,65	20190	0,01514
10	Manganous Ammon. Sul.	2	$Mn(NH_4)_2(SO_4)_2$	55,0	283,28	71,04	20130	0,01509
11	Manganous Nitrate.	2	$Mn(NO_3)_2$	55,0	179,08	114,37	20480	0,01536
12	Ferrous Iodide.	2	FeI_2	56,0	309,70	55,18	17090	0,01282
13	Ferrous Sulphate.	2	$FeSO_4$	56,0	152,06	111,72	16990	0,01272
14	Ferrous Ammon. Sul.	2	$Fe(NH_4)_2(SO_4)_2$	56,0	284,28	59,42	16890	0,01268
15	Ferric Chloride.	3	$FeCl_3$	56,0	162,35	111,14	18040	0,01356
16	Ferric Bromide.	3	$FeBr_3$	56,0	295,88	66,77	19750	0,01482
17	Ferric Sulphate.	3	$Fe_2(SO_4)_3$	56,0	400,18	101,06	2×20200	0,01515
18	Ferric Ammon. Alum.	3	$Fe(NH_4)(SO_4)_2$	56,0	266,20	75,64	20140	0,01510
19	Ferric Ammon. Oxalate.	3	$Fe(NH_4)(S_2O_4)_2$	56,0	250,08	81,75	20450	0,01533
20	Ferric Nitrate.	3	$Fe(NO_3)_3$	56,0	242,12	74,48	18030	0,01352
21	Cobaltous Fluoride.	2	CoF_2	59,0	97,00	142,60	13830	0,01037
22	Cobaltous Chloride.	2	$CoCl_2$	59,0	129,90	108,21	14060	0,01054
23	Cobaltous Bromide.	2	$CoBr_2$	59,0	218,92	63,13	13820	0,01036
24	Cobaltous Iodide.	2	CoI_2	59,0	312,70	44,08	13790	0,01034
25	Cobaltous Sulphate.	2	$CoSO_4$	59,0	155,06	87,65	13590	0,01019
26	Cobaltous Nitrate.	2	$Co(NO_3)_2$	59,0	183,08	76,62	14030	0,01052
27	Nickelous Fluoride.	2	NiF_2	58,7	96,70	61,50	5950	0,00446
28	Nickelous Chloride.	2	$NiCl_2$	58,7	129,60	46,02	5960	0,00447
29	Nickelous Bromide.	2	$NiBr_2$	58,7	218,62	27,00	5900	0,00442
30	Nickelous Iodide.	2	NiI_2	58,7	312,40	18,62	5820	0,00436
31	Nickelous Sulphate.	2	$NiSO_4$	58,7	154,76	37,39	5790	0,00435
32	Nickelous Nitrate.	2	$Ni(NO_3)_2$	58,7	182,78	32,31	5910	0,00443
33	Cupric Chloride.	2	$CuCl_2$	63,6	134,50	16,33	2200	0,00165
34	Cupric Bromide.	2	$CuBr_2$	63,6	223,52	9,40	2090	0,00157
35	Cupric Sulphate.	2	$CuSO_4$	63,6	159,66	13,72	2190	0,00164
36	Cupric Nitrate.	2	$Cu(NO_3)_2$	63,6	187,68	11,54	2170	0,00163

From the series of numbers in the foregoing table we may draw the following conclusions : The molecular susceptibility shows the well-known increase from chromium to manganese and iron and then a gradual decrease to copper. The four halogen salts could be investigated unobjectionably as bivalent salts only, in the case of manganese, cobalt and nickel; there occurs here again a regular increase from the fluoride to the chloride with a following decrease to the iodide. Simple additive or subtractive properties are obviously not present.

The approximate coincidence of the chlorides and nitrates is striking throughout; in connection with the ferric compounds it is noticeable that the bromides, sulphates, oxalates, give values which are 12 per cent. higher than those which the ferrous compounds furnish; this agrees with the results found by G. Wiedemann. Moreover, the difference among the separate salts amounts to 5 per cent., so that the introduction of a fixed atomic susceptibility for the cation cannot be allowed without considerable arbitrariness. That this quantity exerts by far the chief influence upon the value of the molecular susceptibility has, of course, never been doubted. We have investigated the sulphates of six metals. These are suitable for comparison with earlier observed values and with the proposed algebraic relations which are collected in Table III.

TABLE III.

Observer.	Cr'''	Mn''	Fe''	Fe'''	Co''	Ni''	Cu''
Liebknecht-Wills (from exp.)	7990	20190	16990	20200	13590	5790	2190
" " ($Ni''=a$)	$1.38a$	$3.49a$	$2.93a$	$3.49a$	$2.35a$	a	$0.38a$
" " ($b=1.25a$)	——	$3.50a$	$2.88a$	——	$2.25a$	a	——
G. Wiedemann (exp. $Ni''=a$)	$1.34a$	$3.29a$	$2.74a$	$3.26a$	$2.26a$	a	——
Jäger-Meyer: ($Ni''=a=b$)	——	$3a$	$2.5a$	——	$2a$	a	——

Our values for the sulphates of Mn, Fe, Co and Ni may be approximately represented by Wiedemann's proportion, whereby $b = 1.25a$; of course, only two of the latter numbers are independent since Mn and Ni serve to determine the parameters a and b; putting $a = b$ does not give an agreement with our results. Finally, these are with respect to nickel everywhere larger than those of Wiedemann, also than those of Jäger and Meyer; the departure can only be explained in case the nickel compounds investigated by these observers should have contained cobalt; this, in our judgment, must have been the case.

We wish to thank Dr. W. Meyerhoffer for his kind assistance in carrying out numerous analyses. The investigations were carried out in the laboratory of Professor H. du Bois, to whom our cordial thanks are due.

BERLIN, December, 1899.

GIBBS' THERMODYNAMICAL MODEL.

By W. P. Boynton.

In Maxwell's Theory of Heat, p. 207, is a drawing showing some of the principal lines on a thermodynamical model, suggested by Professor J. Willard Gibbs, of Yale University. I have been told that Professor Maxwell had two of these models constructed, one of which remained at Cambridge, England, the other being sent to Professor Gibbs at Yale. There is also a copy of this model at Clarke University, Worcester, the only one which I have seen. While there may be others in existence, these are the only ones which I have known of, and I suspect that very few have ever been constructed.

This year, in connection with a course in thermodynamics, two of my pupils are attempting to construct one of these models, but are met by various serious difficulties, which may interest others.

In the diagram of which I speak, which is here reproduced, the directions chosen for the different coördinates are not immediately evident. Even by the aid of the description in the text I have not been able to locate them satisfactorily. In the attempt so to do, I have been guided by the following general considerations. Using Maxwell's notation, in which

$v =$ volume.
$p =$ pressure.
$T =$ absolute temperature.
$e =$ energy.
$\varphi =$ entropy.

The equation relating these quantities is

$$Td\varphi = de + pdv$$

which transposed gives

$$de = Td\varphi - pdv = \frac{\partial e}{\partial \varphi} d\varphi + \frac{\partial e}{\partial v} dv$$

the differential equation of the thermodynamical surface of which the coördinates are the entropy, volume and energy, and whose slope at each point in the principal directions gives the temperature and pressure, by the identities

$$T = \frac{\partial e}{\partial \varphi}, \quad p = -\frac{\partial e}{\partial v}.$$

These are subject to the conditions that T is always positive, and p

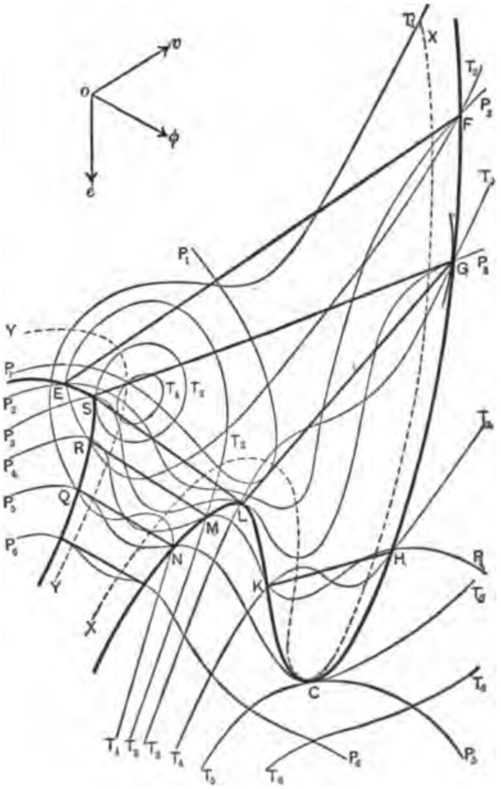

Fig. 1.

is usually positive, always so for the gaseous state, usually for the liquid and solid states.

If then e is taken vertically downward and v and φ horizontal, passing along a section of the surface by a plane of constant volume, in the direction of increasing entropy the slope will always be downward, and generally convex, as the addition of heat, that is energy to a substance at constant volume increases its entropy, and generally its temperature, never decreasing it. A section by a plane of constant entropy will have a slope in the direction of increasing volume which is in general upward, corresponding to a positive pressure, and in all parts of the model referring to stable states of the substance this will be convex, since increase of volume is then accompanied by decrease of pressure.

I have attempted to determine the choice of coördinates by the properties of the critical state. In the two diagrams the broken line separates the parts representing stable or homogeneous states from parts representing unstable or non-homogeneous states.

In the pressure-volume diagram lines of constant pressure, volume, entropy and temperature are drawn. On the other are drawn lines of constant pressure and temperature, taken from Maxwell. In both diagrams these lines are tangent to the broken line. In Fig. 2, the line $v =$ const. cuts sharply through the broken line. I have attempted to find the behavior of a line of constant entropy in the following way:

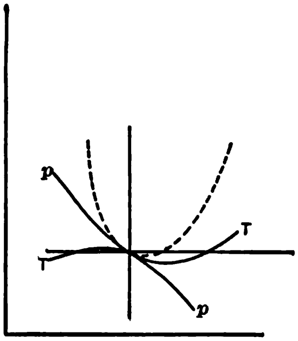

Fig. 2.

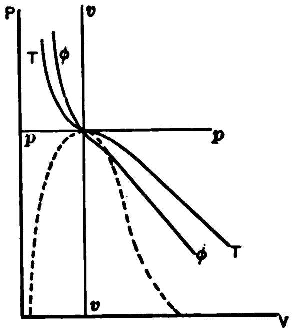

Fig. 3.

For a substance following van der Waals' equation

$$\left(p+\frac{a}{v^2}\right)(v-b)=RT$$

the equation of an isentropic can be shown to be

$$\left(p+\frac{a}{v^2}\right)(v-b)^k = \text{const.}$$

where *k* is the ratio of the specific heats at constant pressure and constant volume. The slope of this curve is found to be

$$\frac{dp}{dv} = \frac{2a}{v^3} - \frac{k\left(p + \frac{a}{v^2}\right)}{v - b},$$

which becomes, substituting the values of the critical pressure and volume

$$\left(\frac{dp}{dv}\right)_{\text{crit.}} = \frac{2a(1 - k)}{27b^3};$$

which is negative for real positive values of *a* and *b*. Hence the isentropic appears also to cut through the broken line, but less sharply.

Still further, we believe that the line of constant volume does not again cut out of this non-homogeneous or unstable area, while the isentropic may. Hence it has seemed to me necessary to consider the vertical line of Fig. 3 a line of constant volume, and the horizontal line an isentropic, while the critical point lies a little to the left of the vertex of the curve, so that the isentropic slightly cuts through the broken line.

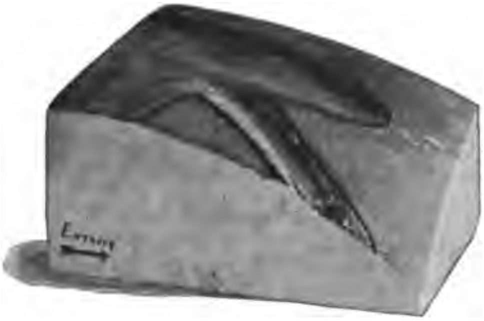

Fig. 4.

The choice of coördinates will then be, energy, vertically downward, in the three-dimensional model, volume, measured to the right, in the diagram, Fig. 1, and entropy vertically downward, in the same diagram. This choice is not inconsistent with the arrows in the upper left-hand corner of Fig. 1. The model which has been constructed in accordance with these considerations is shown in the accompanying figure.

It satisfies the general requirements as to slope and convexity. It represents the gaseous or vapor state as having in general the greatest volume and a great range of pressure, etc. One property, however, does not seem to be indicated by this model, nor do I see how to satisfactorily change it so that this can be done.

It has been deduced mathematically and shown experimentally that if a saturated vapor be expanded adiabatically, or isentropically, it may either become superheated or partially condense to liquid; in fact both phenomena can be shown with one substance, as for instance, chloroform above 127° C. becomes superheated, and below this temperature condenses, by sudden expansion, while at this temperature no visible effect is produced by either expansion or compression. That is, there is an isentropic which is at a particular point tangent to the "steam line," those on one side of it not touching it at all, while those on the other cut both in and out. This particular property is not shown by the model as constructed, with the present choice of coördinates. If, however, we had measured entropy horizontally in Fig. 1, then the isentropics, being vertical, might be tangent to, or cut through the steam line. This choice of coördinates has, however, seemed impossible for the reasons previously given.

We may, if we wish, discuss the question by a different method. The lines drawn in Figs. 1, 2 and 3 are all lines through the critical point. In Fig 2 the lines of constant pressure and temperature are tangent to the broken line; Fig. 3 shows the same property. In Fig. 2 the line of constant entropy cuts the broken line twice, but no other pair of lines has more than one intersection. Fig. 3 does not, as drawn, show the same property. In Fig. 2, passing from the water-line around the critical point in the homogeneous region to the steam-line, one cuts the lines in the following order: water-line, pressure, temperature, entropy, volume, pressure, temperature, entropy, steam-line. Fig. 3 gives the same order, with the choice of coördinates which we have adopted, if we let the temperature lines always slope downwards as due the pressure lines. With this change the two diagrams seem to agree, but otherwise their disagreement seems hopeless.

I shall be very glad to receive from anyone any suggestion which will help to remove the apparent disagreement between the two diagrams, or so modify the model that it may more completely represent the possible properties of actual bodies than it now seems to do.

THE UNIVERSITY OF CALIFORNIA, BERKELEY, CAL., Feb. 1, 1900.

ON THE TEMPERATURE OF THE ACETYLENE FLAME.[1]

BY EDWARD L. NICHOLS.

CONCERNING the temperature of the acetylene flame, varying and incompatible statements are in existence. The temperature of combustion of this gas, according to Le Chatelier,[2] would be, when burned in air, from 2100° to 2420°. Measurements with Le Chatelier's pyrometer, on the other hand, made by V. B. Lewes,[3] gave temperatures lower than those of ordinary gas flames. Lewes found for the obscure zone 459°, for the edge of the luminous zone 1411°, and for the region near the summit of the luminous zone 1517°. Smithells,[4] upon the appearance of the data given by Lewes, described a series of experiments for the purpose of showing that the temperature of the flame is, in point of fact, very much higher than the values given by that author and that in many portions it is higher than the melting point of platinum.

It can be easily shown by inserting wires of that metal into the flat acetylene flame obtained from any one of the forms of burner usually employed, that while the thicker wires remain unmelted, those of very small diameter are readily fused. I found, for example, that a wire having a diameter of 0.0082 cm. became fused at the end with the formation of a distinct globule, before the metal had penetrated the outer luminous layer of the flame, whereas wires of 0.01 cm. or larger diameter remained unmelted. The experiments of Waggener, cited by St. John,[5] show that there are portions of the flame of the Bunsen burner in which it is possible to melt platinum, while MacCrae,[6] working with a platinum-rhodium element found for the hottest region in the Bunsen flame 1725°. It will be seen from the experiments to be described in this paper that MacCrae's determination, which was made with wires having a diameter of 0.02 cm. is not incompatible with the observations of Waggener and others. Smithells, in the paper just cited, describes the melting of platinum wires having a diameter of 0.01 cm. in various parts of the outer sheath of a flat flame of illuminating gas.

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[1] A paper read at the meeting of the American Physical Society, February 24, 1900.

[2] Le Chatelier, Comptes Rendus, 121, p. 1144 (1895).

[3] Lewes, Chem. News, 71, p. 181 (1895).

[4] Smithells, Journal of the Chemical Society, 67, p. 1050 (1895).

[5] St. John, Wiedemann's Annalen, 56, p. 434 (1895).

[6] MacCrae, Wiedemann's Annalen, 55, p. 97.

Pellissier[1] in commenting upon Lewes's measurements refers to experiments in which minute wires of platinum, made by Wollaston's method of silver plating, drawing and subsequent dissolving of the silver coating, when thrust into the flame of a candle, melted instantly. I have not been able to find other printed reference to these observations and do not know with whom they originated. An attempt to repeat the experiment with a Wollaston wire resulted in the ready fusion of the wire by the flame. An examination of the remaining portions under the microscope showed that the metal had been melted down into clean well-rounded beads, and had not been consumed by oxidation or any other chemical reaction.

It will appear from the experiments to be described in this paper that Smithell's contention that the temperature of flames cannot be obtained directly from the indications of a thermo-element, for the reason that such an element loses heat by conduction and by dispersion from its surfaces so that the portions submerged in the flame never arrive at the temperature of the surrounding gases, is well founded. Lewes recognized this fact, and in his measurements made use of wires of three different sizes.

THE APPARATUS.

It is obvious that the indications of a thermo-couple which is submerged in a flame must depend upon the diameter of the wire, and it is upon this fact that my method for the estimation of the temperature of the acetylene flame is based. The flame selected for these measurements was of the usual flat type, obtained by the impinging of two jets of acetylene properly mixed with air, which issued from the orifices of one of the well-known forms of acetylene gas burner. The burner was mounted on a stand sliding upon a horizontal steel bar, so that a free lateral motion could be given to the flame. This stand moved so smoothly upon its metal support that the flame could be shifted by the direct action of the fingers of the operator with the same precision and freedom that we find in the well-adjusted draw tube of a microscope; and lateral displacements of about 0.01 cm., or even less could be made with ease. In some of the experiments requiring more accurate control, the flame was moved by means of a fine micrometer screw clamped to the bar. Upon a similar stand, attached to the same bar, was mounted one of the series of thermo-elements by means of which the determinations in question were to be made. The wires were all drawn from the same samples of pure platinum and platinum-rhodium (10%), obtained from Heraeus in Hanau, from stock believed to be identical with that tested by Holborn and Wien.[1] That thermo-elements made of wires differing in diameter, but all from the same stock, give with sufficient accuracy identical results, has been abundantly established. The hot junctions were given the form

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[1] Pellissier; l'Eclairage a l'acétylène (Paris, 1897), p. 186.

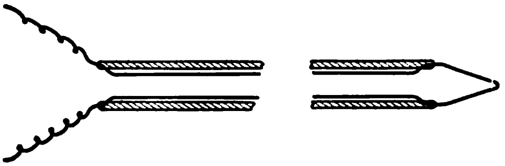

Fig. 1.

shown in Fig. 1. The platinum and platinum-rhodium wires were fastened by sealing into the fused ends of an open glass rod about 10 cm. in length, and 1 cm. in diameter, and the free ends of these wires were cut off, leaving about 3 cm. extending beyond the glass. These ends were pinched together in the form of a V, and while in contact were fused by a brief immersion in the oxy-hydrogen flame. The junction thus formed was trimmed off at right angles to the axis of the tube, until the thickness of the fused portion joining the two wires was as small as could be obtained without danger of rupturing the junction. It was found that by this process of trimming away with a pair of pocket scissors, better results could be obtained than by the use of file, or emery

[1] Wiedemann's Annalen, 47, 107, and 56, 360.

paper, and that the preparation of junctions in this way was very easy and expeditious. It was almost always possible, after a few trials, to secure a junction in which the thickness of the connecting link of fused metal would not exceed 0.005 cm.

When viewed under the microscope, a junction thus prepared presents the appearance shown in Fig. 2. The surface left by trimming is smooth, and lies in a plane at right angles to the axis of the tube in which the wires of the thermo-element are mounted. When such a junction is brought near to the flat flame from an acetylene burner, its plane face lies parallel to the sheaths or layers of which the flame is composed and it is true, with a high degree of approximation, that the whole of the junction is in a surface of equal temperature.

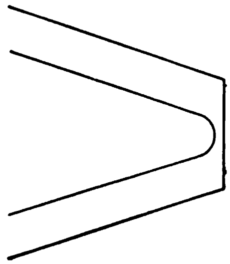

Fig. 2.

The electromotive force produced by the heating of the thermo-element in the flame was measured by means of a potentiometer consisting of a sensitive galvanometer of the D'Arsonval type and an accurately adjusted resistance box containing coils ranging from 50,000 ohms to 1 ohm. A large Clark cell of the old Feussner type was mounted in series with the resistance box. The thermo-element, the galvanometer, and a subsidiary resistance of 10,000 ohms were looped around a portion of the resistance box, the ratios being varied according to the Poggendorff method until complete balance was secured. The electrical connections are shown in Fig. 3. The type of standard cell selected for this work is subject to considerable errors from diffusion lag. It has, however, the advantage of being capable of furnishing a much larger amount of current than the small types of cell in which diffusion lag is avoided,

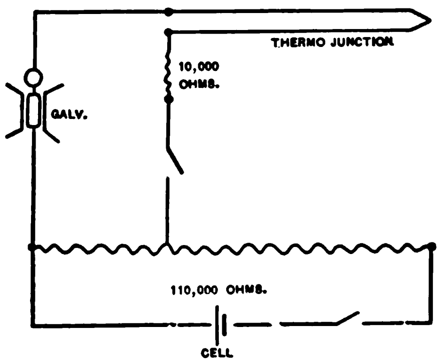

Fig. 3.

without appreciable loss of electromotive force. Two of these cells were placed side by side in a thick-walled inner room which had been constructed for the purpose of securing uniform temperature for the standard clock of the physical laboratory, and other similar apparatus. The range of temperature in this room fluctuated throughout the entire investigation between 18°C. and 19°C. The range was so small and the variations occurred so gradually that no changes of electromotive force of a size which it was necessary to consider in these measurements could have risen, other than those included in the usual correction for temperature.

The two cells were compared with each other from time to time by setting them in opposition to one another in circuit with a sensitive galvanometer and noting the deflection produced. It was found that although one of them was supplying current to the 100,000-ohm circuit of the potentiometer, during the times when it was necessary to close the key of that circuit, the difference of electromotive force between the used and unused cell was always very small, never more than a few hundred thousandths of a volt. At the end of the entire set of measurements, the difference was 0.00006 volt. The absolute electromotive force of these cells was checked by comparison with Clark cells of the H form and of the test-tube form, constructed in this department in 1898. As a result of these comparisons, it was found that the electromotive force of the cell used in the potentiometer might be taken at 1.430 volts at 18°.

For the convenient observation of the acetylene flame and of the distance from the median plane of that flame to the junction of the thermo-element, a long micro-camera was employed, the distance from the lens of which to the ground glass plate was about two meters. By means of a projecting lens of long focus, which was substituted for the ordinary photographic lens usually employed in this camera, an image of the flame and thermo-junction, enlarged to about seven diameters, was projected upon the ground glass. The plane of the flame was parallel to the axis of the camera so that those portions of the image of the former that were in focus were seen as if in cross section.

The stream of acetylene and air issuing at right angles to each other from the openings in the lava jet when viewed in the image, presented the appearance of cylinders or tubes with luminous walls, which under ordinary pressures united after meeting into a single stream of gas, the walls of which were very sharply defined and bright. Outside of this, and swaying back and forth with the drafts of air to which the flame is subjected, was an ill-defined image of the wings of the flame which consist of portions of the gaseous envelope which have lost their velocity and in consequence, the remarkable ability to resist deformation, possessed by the body of the flame. The thickness of the stable portion of the flame, which is surrounded by a well-defined luminous layer was found to be about 0.065 cm. The walls of brilliantly incandescent material around this central body were not more than 0.005 cm. in thickness. The appearance of these essential portions of the image of the flame on the ground glass is shown in Fig. 4, which is an outline sketch from a photograph of the flame. It will be noted that the body of the flame is not vertical, which arises from the fact that one of the jets is slightly stronger than the other. On account of the smallness of the openings in an acetylene burner, it is rare to find complete symmetry in these jets. The flame is almost always slightly thrown to one side as shown in the drawing, one face being convex, the other concave. These slight defects from symmetry do not in any way interfere with the measurement of the flame temperature.

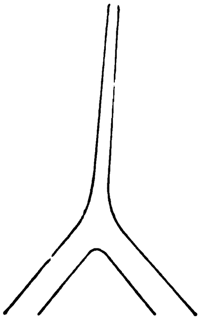

Fig. 4.

Measurements of the Acetylene Flame.

The wires selected for the four junctions to be used in the experiment upon the acetylene flame were measured under a microscope with micrometer stage. Their diameters were as follows:

Table I.

Junction I.	Diameter 0.01996 cm.
" II.	" 0.01598 "
" III.	" 0.01089 "
" IV.	" 0.00821 "

Readings were first made with junction I (diameter 0.01996 cm.). The flame was set at a distance of 6 mm. from the face of the junction and the potentiometer was balanced. The flame was then moved stepwise nearer and nearer, and the potentiometer rebalanced at each step until the face of the junction coincided with the edge of the luminous mantle at a point just above the apex of the inner non-luminous zone. (See *x*, Fig. 5.)

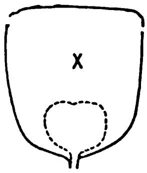

Fig. 5.

The rise of temperature indicated by the potentiometer readings is shown in curve *a* (Fig. 6), the data for which as well as for the other curves in that figure are contained in Table II. The increase of temperature is gradual at first, but at a distance of about 0.4 cm. from the center of the flame, the curve suddenly becomes steep. It is probable that this distance measures the thickness of the layer of non-luminous gas which surrounds the visible flame. Outside of this region the junction is heated almost altogether by radiation. As soon as it penetrates the column of moving gas, however, heat is brought to it principally by convection. Before the surface of the luminous mantle is reached the curve shows indications of approaching a maximum.

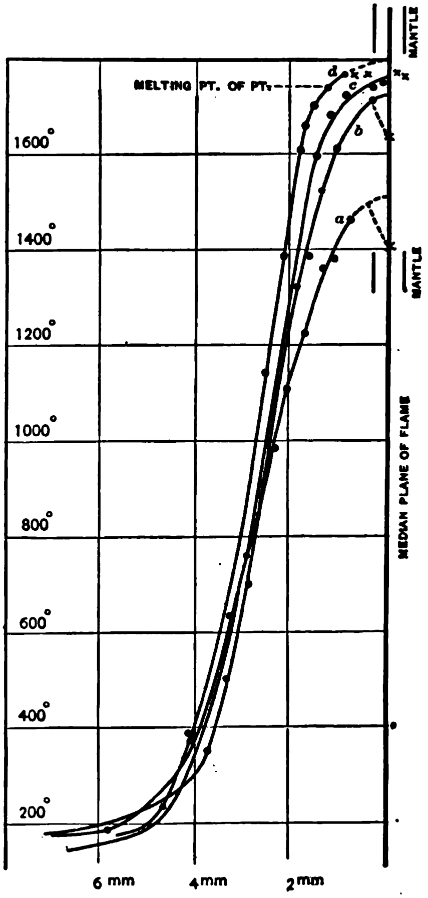

Fig. 6.

Upon pushing the flame still nearer to the junction, so that the latter penetrated the luminous region, an accumulation of lampblack began to form upon the wire, with fall of temperature. This process in the case of acetylene is so rapid that at the end of two minutes a button of carbon several millimeters in diameter is formed. This is finally torn loose from the wire by its own weight; whereupon the deposition of a new mass begins. I attempted, by watching the breaking away of the carbon from the wire, which occurred at regular intervals, to determine the temperature of the wire before the coating of carbon had begun to show itself again. The highest temperature which it was possible to observe in this way was nearly one hundred degrees below that in the luminous layer, and it was obvious from the movement of the galvanometer needle that the junction was being rapidly cooled.

Table II.

Temperatures indicated by thermo-junctions I, II, III and IV, at various distances from the median plane of the acetylene flame.

Junction I.		Junction II.		Junction III.		Junction IV.	
Distance.	**Temp.**	**Distance.**	**Temp.**	**Distance.**	**Temp.**	**Distance.**	**Temp.**
mm.		mm.		mm.		mm.	
5.62	185°	——	——	5.42	165°	4.63	233°
3.91	370°	3.65	353°	4.82	183°	4.11	406°
2.85	760°	3.33	508°	3.21	657°	2.55	1168°
2.09	1128°	2.90	695°	2.03	1278°	2.12	1411°
1.66	1229°	2.30	989°	1.50	1598°	1.86	1613°
1.30	1367°	1.93	1322°	1.18	1685°	1.70	1667°
1.07	1382°	1.68	1385°	0.894	1724°	1.54	1705°
0.850	1467°	1.40	1513°	0.566	1747°	1.30	1738°
		1.09	1617°	0.238	1759°	1.025	1771°
		0.320	1715°	0.00	1775°	0.780	molten
				−0.29	molten	0.300	molten

Junction II. (diameter 0.01598 cm.) was now substituted for junction I., and a similar set of readings were made. This junction, as had been anticipated, showed higher temperatures. It was found possible, owing to the small diameter and consequently high temperature of the wire, to penetrate further into the flame before the deposition of carbon began; so that measurements with the junction actually within the luminous layer could be made. The general form of the curve, as will be seen by inspection of the figure (curve *b*) is the same as that obtained with junction I. After penetrating the luminous mantle to a small fraction of a millimeter, carbon began to gather upon this junction likewise, with lowering of temperature as in the case of junction I. The attempt to read temperatures immediately after the dropping of the accumulated carbon showed that the highest temperature which could thus be observed was again about one hundred degrees below the temperature of the luminous mantle. It was clear in this case, as before, from the rapid fall of temperature already going on, that this reading has no significance.

Similar readings with junction III. (diameter 0.0108 cm.) gave a third curve of the same type as those plotted from the readings made with I. and II.; but the temperatures were higher throughout. With this junction, it was found possible to penetrate to the center of the flame without the deposition of carbon, the temperature of the wire being apparently too high to permit the formation of soot. Upon pushing through the median plane of the flame to the second luminous mantle the junction was melted. This result was not unexpected, since the temperature of the junction at the first luminous mantle reached 1750°, so that a rise of twenty-five degrees of temperature would suffice to produce fusion. The wire when pushed through the flame in the manner just described is heated for greater and greater distances back from the junction until the losses of heat at the junction are sufficiently diminished to raise the tips of the wires to the melting-point.

With junction IV. (diameter 0.0082 cm.) a fourth curve, similar in form to the preceding ones and with still higher temperatures was obtained. This junction was fused at a distance of 0.075 cm. from the core of the flame, and of 0.037 cm. from the edge of the first luminous mantle. It was easy to observe in the enlarged image upon the plate of the micro-camera, the melting away of the platinum wire, while the platinum-rhodium alloy was still unaffected, and while contact was still unbroken. A satisfactory observation of the electromotive force of the thermo-element at the melting-point of platinum was thus obtained. This reading (0.018236 volt) differs from the value found in my calibration of the thermo-junctions used in this investigation (0.018262 volt) by a quantity (0.000026 volt) less than the errors due to changes in the electromotive force of the standard cell. If the latter reading be taken to correspond to 1775° the former indicates 1773°.

Beyond this point it was impossible to make direct observations of temperature; but the form of this and the preceding curves were so closely allied that I felt no hesitation in extending the curve *d* to the core of the flame. This has been done by means of dotted lines in the figure. Curves *a* and *b* have been extended in the same manner. In order to form an estimate of the temperature which would have been reached by a thermo-junction of negligible cross-section, provided such a junction could have been obtained which was capable of registering temperatures above that of the melting point of platinum, the ordinates of the four curves *a*, *b*, *c* and *d* were taken for the core of the flame, for the plane of the luminous mantle, for a plane distant 0.07 cm. from the core, and for a plane 0.10 cm. from the core. These readings were plotted

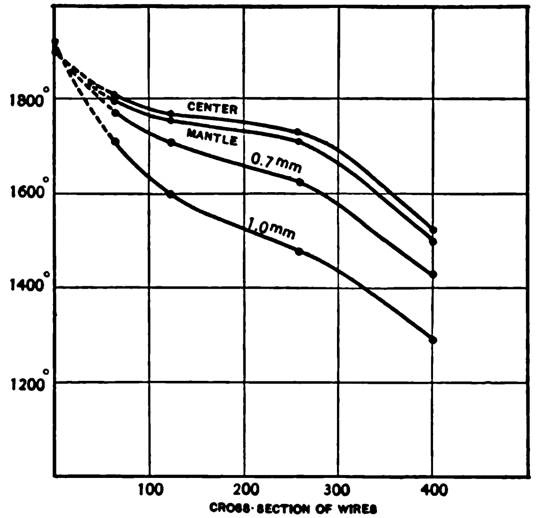

Fig. 7.

and curves were drawn through them as shown in Fig. 7; relative cross-sections of the wires being taken as abscissæ, the temperatures as ordinates. If these curves could be extended to the line representing zero cross-section, the temperatures indicated by the points in which each of them cuts that line would give the temperature of the portion of the flame to which the curve corresponds. There is a considerable element of uncertainty in extrapolation even over so short a range as this; but it is obvious from the character of the curves lying within the limits of observation that each of them trends upward and it seems highly probable that they all meet the line of zero cross-section at a temperature not far from 1900°. The fact that the curves cut this line at nearly the same temperature would seem to indicate that the distribution of temperatures from the center of the flame outward for a distance of about 1 mm. is a nearly uniform one.

It would, perhaps, be unwise to attempt to draw any more definite conclusion from the probable trend of these curves; but I have ventured to extend them in the manner shown in the figure, so that the curve for the region 1 mm. from the center of the flame reaches the zero of abscissæ about twenty degrees above that for the center of the flame, *i. e.*, at 1920° and the intermediate curves at temperatures lying between. I regard this as an extreme treatment of the case, and allude to it only to indicate that, in accordance with common belief, the highest temperature may be found in the outer non-luminous layer of the flame; but that it is unlikely that the difference amounts to more than twenty degrees.

The point of intersection referred to above lies more than one hundred degrees above the highest temperature recorded by even the smallest of the thermo-elements, and it is safe to infer that nearly all previous attempts at the measurement of flame temperatures must, for lack of correction of the error, due to loss of heat through the wire, be regarded as much too low. The Junction IV. is, so far as I am aware, the smallest in cross section that has been used in such work. With larger wires, the correction for loss of heat would be even greater, except in cases where, as in the observations made by Smithells, pains were taken to immerse an extended portion of the wires within the flame.

TEMPERATURE OF OTHER FLAMES.

For the purpose of comparison, I measured in a manner analagous to that just described, the temperature of the luminous flame of ordinary illuminating gas, and the flame of a candle. The gas flame employed for this purpose was obtained from a lava tip, rated at one cubic foot and giving a flat flame of the usual form. The image of this flame, when viewed upon the ground-glass screen of my camera was found to be comparatively ill-defined and unsteady; but although the outlines of the luminous sheath were much less clearly marked than in the case of the acetylene flame, they were discernible. Owing to the continued motion, due to the small velocity of the gas issuing from the jet, no attempts were made to plot curves of temperatures outside the flame. All readings were made with the junction as nearly as possible in contact with the outer surface of the luminous sheath, at a point in the brightest portion of the flame. This position is approximately in dicated by the letter x in Fig. 8. The four junctions already described were mounted, one after another, in such a position that the flame could be moved up until they came into contact with the sheath at the point indicated. The temperatures of the junctions when in that positiona are given in the following table:

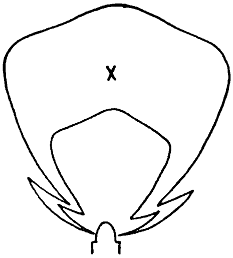

Fig. 8.

TABLE III.

Junction I.	1385°
Junction II.	1484°
Junction III.	1609°
Junction IV.	1676°

These values having been plotted with relative cross sections of the wires as abscissæ, and temperatures as ordinates were found to lie upon a smooth curve (g) as shown in Fig. 9. This curve when extended to the line corresponding to zero cross section gave for the temperature of the flame 1780°, a temperature sufficient to account for the success of Smithells's experiment, already described, in which platinum wires of small diameter were melted in the outer sheath of such a flame. I found it easy, by holding a wire of the size used in junction IV. in a plane parallel to that of the flame, and moving it gradually toward the latter, to verify his statement. The wire was readily melted.

It was not thought necessary to make further experiments upon this flame. The region selected was, so far as one could judge from the brightness of the luminous sheath, the hottest portion of the flame. My measurements upon this region would lead to the conclusion that *the luminous sheath of ordinary gas flames is at least one hundred and twenty degrees lower than the corresponding region in the acetylene flame.* Luminous flames of ordinary illuminating gas would perhaps repay further study, but owing to the fact that such gas is an ever-varying mixture and that it is burned under conditions of pressure, etc., such as to give a fluctuating character to the flame, the problem would have at best an indefinite character from which studies of the acetylene flame are free. In the latter case we have to deal with a definite fuel and the velocity of the jets of gas from the burner is sufficient to give a high degree of stability.

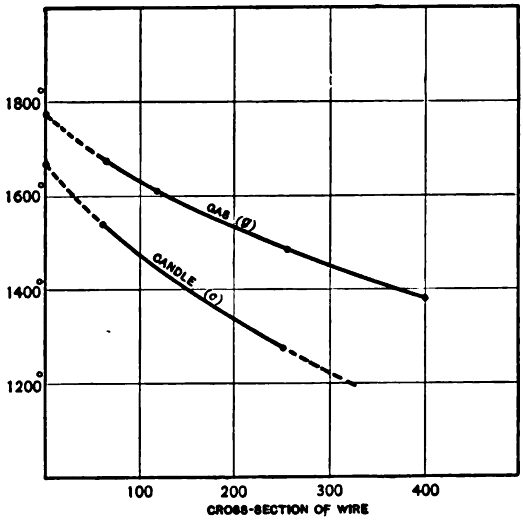

Fig. 9.

The candle would seem an even less satisfactory subject of study in these respects than illuminating gas, but the fact of the melting down of Wollaston wire in a candle flame, the verification of which I have briefly described in the opening paragraphs of this paper, seemed to discredit so completely the lower values commonly assigned to the temperature of such flames that I decided upon a redetermination.

The fact that the flame of a candle, mounted upon a fixed stand, would move steadily downward as the material of which it was composed burned away, made it convenient without any serious modifications of my apparatus, to explore the temperature of the luminous sheath throughout the entire length of the flame. It was only necessary for this purpose to mount a candle upon the steel bar in the position previously occupied by the acetylene flame and when it had reached such a length that the level of the rim of the cup lay below the level of the junction, to move the candle toward the latter by means of a micrometer screw until the junction began to be submerged in the luminous sheath of the flame. It was then easy by a series of slight adjustments of the flame, to explore with the junction the entire surface of the luminous sheath from base to tip, measuring temperatures from time to time, and determining the position by means of the height of the junction above the rim of candle cup. The latter observations were readily made by means of the image of the candle upon the ground glass of the camera. Explorations of the candle flame in the manner described were made with junctions II. and IV. and the results obtained showed a degree of consistency much greater than the fluctuating character of the source under observation had led me to expect. Both sets of observations showed a maximum of temperature in the same region; that lying just above the tip of the interior dark zone of the flame. Readings were made by watching the movements of the candle flame and securing a balance of the potentiometer at times when the face of the junction was as nearly as possible in contact with, but not deeply submerged within the luminous layer. Whenever the wire plunged to any considerable depth beyond the luminous surface, deposition of soot occurred with lowering temperature, and it was necessary to withdraw it into the non-luminous regions outside and to wait until the deposit had been burned off, before proceeding with the readings. In computing the actual temperatures of the luminous sheath of the flame from these readings, I contented myself with the following rough approximation. The maximum temperatures shown by junctions II. and IV. were plotted upon the same diagram used for the luminous gas flame. These temperatures were 1281° and 1546°; values which, as will be seen by inspection of Fig. 9 (*c*), lie much below those of the corresponding readings for the luminous gas flame, but in such positions as to make it easily possible to draw through them a curve analagous in form to that obtained for the latter. Such a curve would cut the line of zero cross section at about 1670° which may, I believe, be taken as the approximate temperature of the hottest portions of the luminous sheath of the candle flame. Estimates of this temperature by the probably less accurate methods of drawing a straight line through the points in question and taking the points in which this line cuts the line of zero cross section to be the temperature of the flame, and estimates based upon the assumption that the true temperature is as many degrees above the temperature indicated by junction IV. for the candle as it is for the gas flame, would lead to values respectively twenty-four degrees and forty degrees lower than that obtained by the method which I have adopted. I believe that the temperature just given (1670°) is much closer to the truth than that obtained under either of the other assumptions. Estimated temperatures for other portions of the luminous sheath were made by assuming that the correction to be applied to the readings obtained with junction IV. would be the same in all positions. These values are given in Fig. 10 which may serve in place of an ordinary table. The portions of the flame to which each reading refers are more readily indicated by giving such a diagram of the flame than in any other way.

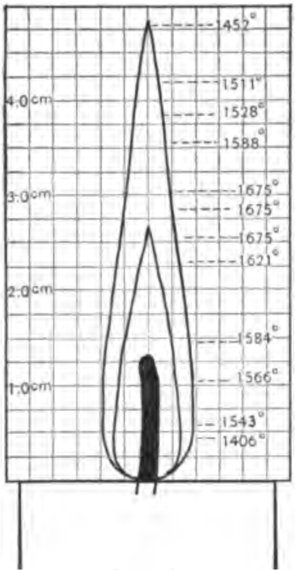

Fig. 10.

The fact that, in the case of the acetylene flame and the ordinary gas flame, this method gives values high enough to account for the melting of platinum, but leads to an estimate of the temperature of the candle flame which is about one hundred degrees below the melting point of that metal, would seem, at first sight, to throw the procedure into serious doubt. My experience with the method has, however, been such as to make an error of one hundred degrees in the estimation of the candle-flame temperature seem highly improbable. Messrs. Lummer and Pringsheim in a recent communication to the German Physical Society[1] give an estimate of the temperature of candle flames based upon a relation which they have established between the position of the maximum in the energy curve of the spectrum of a source of light and its temperature. Assuming the the radiating substance in the flame to have the properties of a *black body* they find this temperature in the case of the candle flame to be 1687°, a value but seventeen degrees above that which I have given.

To account for the fusion of Wollaston wire in the flame of a candle, one might consider the possibility of the existence in such a flame of layers of gas the temperature of which is much above the surrounding regions, and that these layers may be so thin that it would not be possible to submerge the thermo-junction completely in them. In such a case, the junction would give a value approximate to the average of the temperatures of the gases with which it was brought into contact. Before assuming this structure of the flame, which really has nothing to support it save the necessity of accounting for the apparent discrepancy which I have just pointed out, it seemed wise to consider, on the other hand, whether the melting point of the Wollaston wire was necessarily that of pure platinum. Such wires would be made of ordinary commercial metal, the melting-point of which might vary considerably from that of the purer platinum used in the determination of the temperature of fusion. It is likewise readily conceivable that in the process of drawing within the silver coating, a certain amount of silver might be worked into the pores of the platinum and not removed by the subsequent action of the nitric acid. The determination of the melting-point of even such minute wires is fortunately a simple matter by means of the form of thermo-element used in the experiments which I have just described. It is only necessary to wrap a piece of the wire to be tested around the junction as shown in Fig. 11, to cut it off so that the ends of the loop extend slightly (about 0.05 cm.) beyond the face of the junction; and having mounted the junction in the usual manner, to move the acetylene flame up to it by means of a micrometer screw, watching the enlarged image of junction and flame upon the ground glass of the micro-camera. The wire will then begin to fuse at its outer end, and the fusion, following upon the progressive movement of the flame, can be watched until the loop is melted back to the surface of the junction itself. With such precision can this progressive melting of the wire be carried out in a room free from draughts, that it is possible to melt away the loop of wire in front of the junction until no part of it protrudes beyond the smooth face of the latter, and yet leave that portion of the loop which extends behind the wire of the junction and is protected by it, unmelted. I performed this experiment with a piece of the same Wollaston wire which I had succeded in melting in the candle flame; and I found its melting point, as indicated by the electromotive force of the junction, to be 1674°. To test the question whether this very low melting-point was due to the presence of silver undissolved by the nitric acid, a piece of the same wire was left in the acid for twelve hours, after which the melting-point was again tested in the manner just described. The result of this determination was 1687°. The latter reading was, I think, too high, since subsequent examination under the microscope showed that the loop of the wire behind the junction had been melted, so that the junction was probably a few degrees too hot. At any rate, it may safely be concluded from these determinations that the melting point of the Wollaston wire was at least one hundred degrees lower than that of pure platinum, and that the fusion of such wire in the candle flame did not necessarily indicate that my estimate of the temperature of the flame was at fault.

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[1] Lummer and Pringsheim; Verhandlungen der Deutschen Physikalischen Gesellschaft; 1899, p. 214.

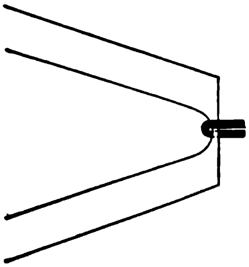

Fig. 11.

This point I deemed it worth while to confirm by direct observation of the melting-point of the Heraeus platinum; for which purpose a piece of the wire used in junction IV. was rolled into a flat strip. From this strip a piece about 0.01 cm. broad was cut, which was looped around the junction in the manner just described. The melting-point was found to be 1775°. Experiments upon the fusion of gold, platinum, palladium and other metals made in this way, have convinced me abundantly of the delicacy and reliability of this means of determining melting-points. It is free from many of the most serious difficulties involved in the procedures usually employed. The details of this method, which is especially applicable to the calibration of thermo-elements, I propose to describe at some length under a separate title.

Scale of Temperatures.

The temperatures given in this paper are based upon a scale arbitrarily adopted, in which the melting-point of pure platinum is taken at 1775°, and gold at 1070°. Nothing more is claimed for this scale than that the reference points are those most easily reproducible in high temperature work, so that any one having occasion to use the data given can make reductions to whatever scale of temperatures he desires to employ. Up to the present time, high temperature work has not reached a stage in which it is possible to do more than to refer readings to these melting-points. The assignment of the proper degrees centigrade to the melting-points of gold and platinum appears as yet to be a somewhat uncertain matter, but the behavior of platinum—platinum-rhodium thermo-elements appears to be in so far consistent, that when the temperatures of these two points are fixed, but little doubt need be felt concerning the relative values of the intervening temperatures. Various wires even from the same maker will give electromotive forces that differ slightly one from another, but in general it may be said that with pure platinum—platinum-rhodium (10%) elements from a given stock of metal, the character of the curve will always be closely the same. Since my materials were from the same source as those used by Holborn and Wien, I have contented myself with the use of their curve, merely shifting the ordinates so as to make it pass through the points corresponding to the electromotive forces at the melting-points of gold and platinum as determined by myself. Various calibrations made in the course of other work appear to justify this procedure. These calibrations I hope to discuss at considerable length in a forthcoming paper. Whatever changes it may in future be found necessary to make in the assignment of these fixed points can readily be extended to all my data, by any one who may care to do so.

To Mr. L. W. Hartman, who aided me in making many of the observations described in this paper, I desire to express my indebtedness.

PHYSICAL LABORATORY OF CORNELL UNIVERSITY, February 21, 1900.

NOTE ON THE LAW OF DISTRIBUTION OF VELOCITIES AMONG GAS MOLECULES.

By N. D. C. Hodges.

LET us consider one molecule. Let this molecule pass from a point A, to a point B, the time of passing being such that the molecule shall have come in contact with as many molecules as there are in the unit of volume. By taking a unit of volume large enough, the molecule considered will, when it reaches B, have passed through all possible velocities, and through each velocity in proportion to the frequency of that velocity among the molecules, *i. e.*, according to the law of distribution, if such exists.

The system consisting of the single molecule and all the other molecules is a conservative system. The sum of the kinetic energy of the single molecule and that of all the others, remains constant. The principle of least action applies.

Any particular velocity being represented by c, the number of times the molecule will have a velocity between c and $c + dc$ in passing from A to B, will depend on N, the total number of molecules in unit of volume, on some function of c, $\varphi(c)$, the form of which is to be determined, on dc, and on a constant A dependent on the temperature of the gas and the molecular weight.

The kinetic energy at any instant will be expressed by $ANc^2\varphi(c)dc$ and the action by $AN\int_0^\infty c^2\varphi(c)dc$.

That this may be a minimum, $\varphi(c)$ must have the form $\frac{\sqrt{h}}{\sqrt{\pi}}e^{-hc^2}$. Hence the expression for the action becomes

$$\frac{AN\sqrt{h}}{\sqrt{\pi}}\int_0^\infty c^2e^{-hc^2}dc.$$

As the variation in the action due to changes of temperature is involvde in A, this expression is solely dependent on the number of velocities the molecule passes through, *i. e.*, it equals N. $\therefore A = 4h$. And the law of distribution is

$$\frac{4Nh^{\frac{3}{2}}}{\sqrt{\pi}}e^{-hc^2}c^2dc.$$

Harvard University, Cambridge, Mass.

NEW BOOKS.

The Elements of Physics. By HENRY CREW, PH. D. Pp. xiv + 347. The Macmillan Co., New York, 1899.

It is not often that the reading of an elementary text-book of physics can be referred to as distinctly refreshing, and yet in the case under consideration this is exactly true. The author writes in so lively a style, with so much enthusiasm for the subject, and with so appreciative a recognition of the value of the experience that the average boy has at his disposal in beginning the study of physics, that the attention is arrested at the outset.

The usual order of development of the subject is followed, but the treatment is by no means commonplace. In the discussion of a topic the author usually follows the order of the historical development of the subject, as being in most cases the natural approach. There is a notable exception to this, however, in the treatment of mechanics, where the usual method is followed, of beginning with motion and later taking up cases of equilibrium. It seems to us that in introducing *beginners* to so difficult a subject as mechanics, we would do well to follow the guidance of history and begin with questions of equilibrium before taking up problems involving acceleration with their attendant difficulties. Of course the method adopted has advantages when the object in view is the systematic presentation of the subject, and this is, doubtless, why it is so generally employed in spite of its disadvantages from a pedagogical standpoint.

The excellent plan has been adopted of giving the names of distinguished physicists in connection with their discoveries, and with each in brackets the dates of birth and death; thus almost unconsciously the student comes to have a historical perspective of the subject. The author's interest in the history of the subject leads him to reprint the very interesting original account of Boyle's experiments on the "Spring of the Air," and in the case of Torricelli's experiment to come to the defense of the old *horror vacui* theory in a way that is quite amusing.

A treatment of water waves that is unusually complete for an elementary text-book is an excellent feature. The change of front due to retardation in shoal water is discussed and used to advantage later in considering the refraction of light waves. It seems to us that in most of our text-books the subject of surface waves receives very scant treatment, considering its importance.

In treating the subject of electrical resistance the author prefers to approach it from the heating effect of the current, and does not take up Ohm's law till almost the last topic in his treatment of currents, though it would seem to us that the early introduction of the point of view of electric currents expressed by Ohm's law might be a help to the student.

Special references to other books are given at the ends of some of the chapters, with comments as to the character of the treatments referred to. These will doubtless be very helpful to a student wishing to go more extensively into some part of the subject.

The whole treatment, though elementary, is exact and quantitative, and numerous problems are given. The student who masters this text-book will have to *think*, but he will find himself stimulated and helped in his thinking by a natural presentation of the subject, by clear statements, lucid explanations and apt illustrations.

The general arrangement is clear and systematic, good use being made of different styles of type in emphasizing important points.

On the whole the book is to be distinctly approved as a freshly-written modern presentation of the subject, elementary, but exact and thorough, and well adapted for advanced high school instruction. The teacher will find in it many valuable suggestions. The author is a teacher of experience, who has evidently given thought to the art of presenting the subject so that it may be most readily grasped by the pupil.

ARTHUR L. KIMBALL.

A Text-book of Physics. By W. WATSON. London, Longmans, Green & Co., 1899. Pp. xxii + 896.

This addition to the rapidly growing list of physics text-books contains many notably commendable features. One recognizes in it at once the work of a skillful writer, an experienced teacher and a working physicist. Mr. Watson's volume is comprehensive in the sense that it states nearly every essential principle of physics, says something on all of the important phenomena, and describes a great number of the methods by which our knowledge of the leading facts of the science has been attained. There is likewise a wealth of well selected tabular data interspersed throughout the book. The volume is bulky, consisting of nearly 900 pages of closely printed matter, but when we turn to any given topic we find that its bulkiness is not due to the introduction of any considerable amount of detail in the description or in the theoretical discussions, but is simply a symbol of the enormous amount of material which has to be included in any treatise covering the domain of modern physics. The individual topics indeed are very briefly considered, and one is inclined to quarrel with them on the score of omission rather than because they are overloaded with unnecessary matter. The author's statements are for the most part clear, and in accordance with the accepted views of physicists.

One finds, as he must expect to find in any such treatise, minor points concerning which he would take issue; as, for example, where electromotive force and difference of potential are specifically stated to be different names for one and the same thing, or where, as in the diagram on page 516, wave-lengths of light are expressed in centimeters multipled by 10^{-10} (the minus sign in the exponent is omitted; a misprint that is not likely to mislead any one), instead of using, as elsewhere in the volume, one of the two accepted systems. It is true that the reference of wave-lengths to a unit thus simply related to the centimeter might well be preferred to either of those which have come into use, but it is a question whether in a text-book it is well to depart from the general practice, especially as the author in subsequent pages reverts to the beaten path.

These, however, are matters of little moment. The important thing is that Mr. Watson has produced for us a treatise on experimental physics, which contains brief and accurate statements of so many of the important topics in physics. The value of the book as a reference work would have been greatly increased by the introduction of citations of original sources, and this remark applies particularly to the well-selected tables of physical constants. Many of the illustrations are new, and some of them will be welcome additions to the store of illustrative material already at the command of the teacher.

We are glad to note what is really very unusual among British writers, even of the modern school: that is, a fairly consistent adherence to the gram, the centimeter and the second as fundamental units.

E. L. N.

Volume X. *May–June, 1900.* *Number 5.*

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THE

PHYSICAL REVIEW.

THE TRANSMISSION OF THE IONIZED EXHALATIONS OF PHOSPHORUS THROUGH AIR AND OTHER MEDIA.[1]

By C. Barus.

I. The experiments of the present paper have been made by an electrical method. They relate to the decay of the ionization of phosphoric dust in the lapse of time, and to the transmission of the same through layers of air, or other media, or barriers. I hope to decide whether a form of radiation is presumable, or whether the case is merely that of an ionized gas exhaled by slowly oxidizing phosphorus. The results which I have obtained are remarkably similar to the behavior of thorium recently investigated by Rutherford,[2] and the theory of the phenomenon might be given in accordance with the better established principles as he has done. I shall venture, however, to treat the case in a simple and direct manner, in order to present it more consistently with my last paper[3] on the same subject, in which the attempt was made to arrive at the ion velocity of phosphoric dust by a non-electrical method.

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[1] Communicated with the permission of the Secretary of the Smithsonian Institute.

[2] Phil. Mag. (5), XLIX., p. 1, Jan., 1900.

[3] Science, XI., p. 201, Feb. 9, 1900. The ionization of phosphoric dust was known to Matteuci, and has been studied since by Neccarri and by Bidwell. I believe to have been the first to point out its remarkable activity in producing condensation, and the substance is specially interesting to me because of this property. Cf. Bull., No. 12, U. S. Weather Bureau, 1895.

Let k be the ion velocity of the phosphoric dust particle, normally to a charged wall, A. The prism of phosphoric air which reaches A in one second will, for any appreciable length in the direction of k, be at an average potential zero, and its successive layers will on the average show no charge, although saturated with the ionized agency stated. Considered non-statistically, however, the individual sections at molecular distances apart must convey immensely different charges successively, the distribution of charge or of potential on successive sections following a law something like Maxwell's, for instance, relative to the distribution of velocity among molecules in the kinetic theory of gases. To deal with the problem in this broad form would make it needlessly cumbersome, without conducing to the present purposes. Apart from this, it seems possible to obviate the question of distribution somewhat as follows:

Suppose the distribution of potential in the direction of k is enormously variable as compared with the potential of A, in such a way as to give preference neither to positive nor to negative values. A will lose charge if its potential is instantaneously greater than that of the section which meets it; it will receive charge in the opposite case. A at potential zero, therefore, neither receives nor loses charge, since the number of sections reaching A in an appreciable time carry equal and opposite charges in like distribution. If the charge on A is positive there must then be fewer layers which impart charge to A and more layers which withdraw it than in the preceding case, since the average charge on the layers is still zero. Hence A will be discharged in the lapse of time and this more rapidly as its potential is higher. Precisely the reverse will happen if A is negatively charged. Hence to avoid the law of distribution specified I suppose that the charge per square centimeter on A is relatively so small that if it is increased n times, there will be n times fewer layers to charge it and n times more layers

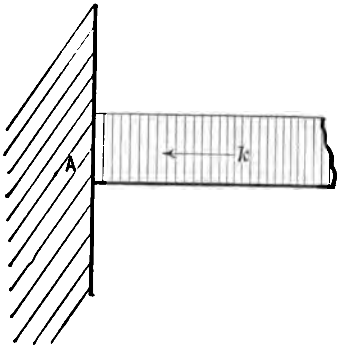

Fig. 1.

to discharge it than in the initial case. In other words I regard the charge on A per square centimeter small enough to correspond to a linear element of the law of distribution of charges along the length of the prism k. Thus the rate of A is caet. par. proportional to Q, its excess above the average charge of the advancing ionized sections which merge into A.

If n be the total number of ions per cubic centimeter, and e is proportional to the charge (positive or negative) carried by each, $k\;n\cdot e$. is the total quantity of free electricity of both kinds promiscuously carried to A, per second per square centimeter. The rate of discharge of A is thus $-dQ/dt = Q\cdot n\cdot k\cdot e$, where the constant of proportionality is contained in e. The integral of the equation is $Q = Q_0\varepsilon^{-knet}$ and the potential of the charged plate therefore

$$\varphi = \varphi_0\varepsilon^{-knet} \qquad (1)$$

in the lapse of time, t, after charging to the initial potential φ_0.

If instead of the plate A, a cylindrical surface of radius r be given like the core of a tubular condenser, for instance, the number of particles reaching the surface per square centimeter per second is $n(k + k^2/2r)e$. Thus $-dQ/dt = Q\cdot n(k + k^2/2r)e$, and

$$\varphi = \varphi_0\varepsilon^{-n(k+k-2r)et}. \qquad (2)$$

If r is infinite equation (1) is reproduced.

Corresponding equations follow for the case of charging the electrometer; but as for practical reasons my work is confined to discharging condensers, further mention is superfluous here.

The conveyance of charge into the ionized region would be similarly explained, virtually in the usual way, following Clausius. Through any interface in the ionized region two such prisms may be imagined, traveling in opposite directions. They travel to and from a boundary. The motion of the prism k is an abstraction; but if I accentuate it here, I do so because in the present investigation with phosphorus it may run closely parallel to the actual state of things. When the phosphorus grid is placed on a smooth clean surface, the position of the discs is soon marked by apparent grease spots, due to deliquescing phosphoric oxide which has diffused across. Virtually therefore an outgoing current, originating in the phosphoric source is continually kept up, whether electricity is demonstrably conveyed or not.

In the annexed table, I have given an example of the results, which is sufficient, inasmuch as the large number investigated present throughout the same character. The following figure shows the method of experiment.

B_2 is a water battery of 48 volts, permanently charging the quadrants of an Elliott electrometer, one of which is always earthed as controlled by the switch S_2. B_1 is a storage battery (20 cells suffice), one pole of which is kept earthed as determined by the switch S_1, to be closed momentarily in charging. The other terminal charges the two condensers in parallel, MN in the electrometer, and CP for the ionization experiment. The plates M and P are also permanently earthed. N communicates in the usual way, with the needle of the electrometer, which is thus at the same potential as the plate C. P is a phosphorus grid, consisting of two sheets of wire gauze facing each other and placed closely together so that between them thin discs of phosphorus may be secured. As the air has free access to P on all sides, the medium between C and P is heavily charged with phosphorus dust. The essential precautions to be observed in work of this kind will be given elsewhere. Barriers are placed for examination between C and P quite out of contact with the former plate.

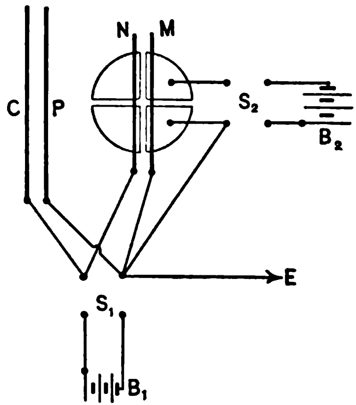

Fig. 2.

Table I. shows the leakage through the condenser CP when the plates are $d = 4$ centimeters apart, a relatively large distance. The exponential character of the law of discharge is manifest, but the decrease is throughout faster than can be reproduced by a simple geometric progression of the kind premised above. This might plausibly be ascribed to the waning activity of phosphorus itself in the lapse of time, and hence $c = n \cdot k \cdot e \cdot \log \varepsilon$ is necessarily a decreasing quantity as n, the number of particles per cubic centim. is sup-

TABLE I.

Permanence of phosphorus emanation.

Absorbent Medium.	Distance apart.	Time		Observed Potential Difference.	Computed Potential Difference.	$c = nke \cdot \log e$
		d	*t*			
Air.	4 cm.	10 h. 0 m.	0 m.	40.0 volts.		.0007
		10	10	39.0		
		15	15	39.0		
		20	20	38.6		
Air saturated with phosphoric dust.	4 cm.	10 h. 27 m.	0 m.	39 volts.	39 volts.	.0360
		28	1	35	36	
		29	2	32	33	
		30	3	29	31	
		31	4	27	28	
		32	5	25	26	
		33	6	23	24	
		34	7	21	22	
		35	8	20	20	
		36	9	19	19	
		37	10	17	17	
		38	11	16	16	
		39	12	16	15	
		40	13	15	13	
		46	19	13	—	
		51	24	13	—	
		57	30	9	—	

plied at a diminishing rate by the phosphoric source. Direct experiments negatived this supposition except for small distances, as the following examples chosen at random from many results of the same kind demonstrate.

TABLE II.

Decay of the ionizing activity of phosphorus.

Time	2h. 44m.	2h. 44.5m.	2h. 45m.	2h. 45.5m.	$d = 3$ cm.
Potential difference	35.6	32.4	29.4	26.8	(volts)
Time	5h. 22m.	5h. 22.5m.	5h. 23m.	5h. 23.5m.	$d = 3$ cm.
Potential difference	35.4	32.4	29.4	27.0	(volts)

According to the above table the rate of discharge of the freshly charged condenser before and after an interval of nearly 3 hours is not discernably different. The successive values of c were

Time	2h. 45m.	2h. 56m.	3h. 17m.	3h. 48m.	4h. 18m.	4h. 46m.	5h. 23m.
$c =$	.081	.078	.086	.078	.078	.070	.080

Fluctuation of temperature and air currents account for these irregularities. With plates 1 centim. apart the values of c were

Time	10h. 20m.	26m.	37m.	47m.	60m.	11h. 28m.	(12h. 1m.)
$c=$	.39	.39	.37	.34	.35	.34	(.33)

The last datum was obtained on readjusting the plates. Fluctuations here are greater, amounting to .05/68 or .0007 per minute, while the value .0009 is found in connection with other experiments below.

Thus it is improbable that in 15 minutes or less (usually less than 5 minutes), which outlasts the time of a single series of observations there should be any appreciable diminution of the ionizing potency of the phosphoric source. As I have not made up my mind on a satisfactory explanation I give below an exhibit of the inadequacy of the simple geometric equation. Let the observations be separated into two halves, and the constant c be computed from corresponding members, φ, t, and φ', t', of the two series. The values c and t of the table show the datum so obtained, and the mean time for which it holds. If the process is repeated to get the ratio of increments δc and δt of the values just found, the data $\delta c/\delta t$ of the table follow. These ratios are practically constant in the same series, but differ widely in different series, *i. e.*, with c. Let the mean c corresponding to each ratio be found as in the table. Then if $\delta c/\delta t$ be computed a datum is obtained, which though fully independent of c, is restricted to narrow limits. Thus in place of the above equation there follows a double exponential law

$$\varphi = \varphi_0 \varepsilon^{-N_\varepsilon - \gamma t' \log \varepsilon}$$

expressing that some independent action has for the time t' been decreasing the number of particles at the rate γ. If C corresponds to c as N corresponds to n, $C/c = 10^{\gamma t' \log \varepsilon}$. The last column shows the values of C found in this way. Compared with c they are constant enough to plausibly suggest the action of some independent cause of decay, such as one would naturally attribute to the source. As this is untenable, one may note that the existence of a countercurrent, or the occurrence of a species of polarization is not unreasonably suggested.

Table showing the insufficiency of a simple geometric progression. Plates 4 centim. apart.

$t-\frac{t_1+t_2}{2}$	c	$\delta c,\delta t$	Mean c.	Mean t.	$\gamma-(\delta c,\delta t);c$	C
4	.0374	.0023	.0327	10 h. 31 m.	.071	.0374
5	348	18	312	32	59	376
6	331	24	254	34	93	387
7	309	20	222	36	93	393
8	280					384
9	275					406
12	177					346
15	136					340
10	.0076	.0005	.0059	11 h. 51m.	.085	.0076
13	55	3	45	54	60	73
17	42					73
21	35					87

2. I next investigated the change of n, the number of particles per cubic centimeter with the distance between the condenser plates. The farther away the ions move normally from the phosphorus grid, the fewer will have survived. As at first trial I will suppose that the same relative number of ions decays per second in each layer. Thus $-dn/dx = an$ whence $n = n_0\varepsilon^{-ax}$. If $c = n \cdot k \cdot e \cdot \log \varepsilon$ as given above, $c/c_0 = n/n_0$. The following table is a summary of a completed series of experiments computed in this way. Since for

TABLE III.

Transmission of phosphoric dust in air. Summary: $c = nke \log \varepsilon$ $c/c_0 = n/n_0$ $n = n_0 10^{-ax}$ $a = .25$.

x	Mean time.	Corrected.[1]		Corrected c observed.	Corrected c computed.	10^{-ax}
		c	c'			
1.7 cm.	11 h. 18 m.	.200	.200	.200	.200	.376
5.7	22	.023	.196	.024	.020	.038
2.5	32	.120	.187	.128	.126	.237
11.5	43	.002	.178	.002	.001	.001
1.8	12 h. 3	.130	.160	.162	.188	.355
3.7	14	.033	.150	.044	.064	.119
1.7	25	.140	.140	.200	.200	.376

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[1] The time correction is here made linearly. At $x = 1.7$, the value of c fell from .20 to .14 in 67 minutes, *i. e.*, .0009 per minute. This result is referred to above in relation to Table II.

any two values of x, c/c' is a constant, the observed value c is to be referred to a fixed value c' ($x = 1.7$ centimeter) corrected for the apparent decreased intensity of phosphoric ionization above referred to. These values c' are given in the table as well as the values c reduced in terms of them.

The table deduces the mean value of a to be .25, whence it follows that at $1/a = 4$ centimeters, the number of ions which still survive is but 1/10 of the original number. Similar ratios $n/n_0 = 10^{-ax}$ are given in the last column. For $x = 1$ cm., 5 cm., .1 cm., the relative numbers are $n/n_0 = .56, .75, .94$ showing how rapidly saturation is reduced even close to the phosphorus grid.

The above table exhibits the general character of the phenomena fairly well. It is, however, theoretically more probable (following the suggestion originally given by Laplace in his theory of capillarity, and recently by Van der Waals in the kinetic theory of gases) to suppose the rate of decay to vary as the square of the number of ions per cubic centimeter. In other words, $-dn/dt = \beta'' n^2$ or since $c = kne \log \epsilon$, $1/c - 1/c_0 = \beta'' x/ke \log \epsilon$. Hence $c = c_0/(1 - \beta x)$, where $\beta = \beta'' c_0/ke \log \epsilon$.

I have given the constants so obtained in the following table:

TABLE IV.

Summary: $c = c_0/(1 + \beta x)$; $c/c_0 = n/n_0$; $c_0 = -.100$; $\beta = -.88$.

x	c	Corrected c observed.	Corrected c computed.
1.7	.200	.200	.200
1.8	.130	.162	.172
2.5	.120	.128	.083
3.7	.033	.044	.044
5.7	.023	.024	.025
11.5	.002	.002	.011

Hence the equation $1/c - 1/c_0 = \beta x/c_0$ where $\beta/c_0 = 8.8$ is closely in accord with the observations, but the result for small values of c is absurd, since c_0 is negative. Inasmuch as the errors in the last two tables are cumulative, better results than this were not to be expected, seeing that a reason for the apparent decay specified has not been made out. Taking the observations at their face value preference must be given to the exponential first discussed. Neither

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ASTOR, LENOX AND TILDEN FOUNDATIONS.

of these expressions, however, can be expected to represent the case fully, for in both differential equations all allowance for those particles which escape laterally from between the plates, is left out. The term ignored may be much more than a correction; but it would be premature to approach the question in this form before the phenomenon as a whole is better understood. Moreover, experiments with a small phosphorus grid at potential zero facing a relatively large charged plate, would offer a disposition better adapted to facilitate computation. Here the lateral loss may be made negligible.

3. The endeavor is finally to be made to decide whether the ionized particles exhaled by phosphorus are accompanied by some form of obscure radiation, or whether the reaction is restricted to an oxidation, the products of which escape at first in virtually a gaseous form. This is best done by placing barriers of thin material between the plates of the condenser, care being taken to prevent the phosphoric dust from passing around the barrier, yet allowing sufficient space for the access of air. It was my plan to fold the sheets box-like around the phosphorus grid kept at potential zero, so that escape around the edge would require a passage of 8–10 centims. to reach the other plate. This suffices, and the

TABLE V.

Transmission of phosphorus ions through barriers.

Barrier.	Thickness.	*c*	Date.
Tissue paper.	.006 cm.	.092	Feb. 3.
" "	"	.018	" 4, A. M.
" "	"	.007	" 4, P. M.
Writing paper.	.010 cm.	.072	Feb. 3.
" "	"	.002	" 4, A. M.
" "	"	.002	" 4, P. M.
Filter paper.	.020 cm.	.011	Feb 4, A. M.
" "	"	.008	" 4, P. M.
Aluminum foil.	.003 cm.	.003	Feb. 4, A. M.
" "	"	.002	" 4, P. M.
Insulation.		.002	
None.		.500	

plates may then be placed quite closely together on opposite sides of the barrier. Energetic action is secured at a distance apart of about 1 centim.

Porous papers, like tissue or filter paper, transmit the emanation in small quantity, depending on circumstances not easily made out. Usually not more than a few per cent. of the emanation will pass even under favorable conditions. The large values found for writing and tissue paper on the first day, were not again obtained[1] in many subsequent experiments. They may be errors, though careful inspection of the results has revealed none. It is also possible that the emanation from phosphorus may be different under qualitatively different circumstances, apart from intensity.

The general result obtained is thus definitely against any obscure radiation. The emanation passes in quasi-gaseous form with great difficulty and loss even through very thin porous barriers. It is stopped by impervious media however thin. Oiled tissue paper cuts off the ions unconditionally. Permeability seems to decrease in the lapse of time, apparently as if deliquescing phosphoric acid clogged the pores.

4. In conclusion I will briefly advert to experiments made to detect ionization in other dust producers. The method selected consisted simply in blowing dust-laden air between the plates of the condensers, the current entering at the center of the earthed plate and escaping at the edges. Thus 86,000 cubic centims. of air bubbled through concentrated sulphuric acid in about 6 minutes produced an electric leakage equivalent to but $c = .0005$; while the leakage of the condenser before the experiment was $c = .0010$. Hence the effect was increased insulation to the exclusion of all traces of ionization. The same air current tested in the color tube gave an intensely opaque field, showing its condensation producing potency to be of high order.

Ammonic polysulphide tested in like manner gave similarly negative results. About 80,000 cubic centims. passed in 5 minutes through the condenser showed no leakage whatever, whereas, the insulation leakage was originally equivalent to $c = .0006$. In this case, however, the air current produced only just perceptible darkening in the field of the color tubes, evidencing therefore but slight condensation producing tendency. The effect of this reagent on the color tube is in general very fleeting. When evaporating from the stopper of the bottle it is intensely active but only momentarily, as a rule. If however the current of air is replaced by a current of coal gas (which is itself somewhat dust-laden, as appears when the test is made with the color tube), the effect is much more persistent. Indeed the air of a large room if rendered impure in this way, reacts on the color tube for a longer interval than under any other easily producible conditions which I have found. The fleeting nuclei from ammonium sulphide seem therefore much more stable when preserved out of contact with an oxidizing medium like air, and the same is true for sulphuric acid in a measure, and for sulphur itself.

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[1] The large values for tissue paper and larger values for filter paper ($c = .05$ to $.06$) have since been obtained under trustworthy conditions. Possibly the emanation from phosphorus may not be homogeneous in relation to temperature.

Brown University, Providence, R. I.

AN HERMETICALLY SEALED TYPE OF CLARK STANDARD CELL.

By H. T. Barnes.

Of the different types of Clark cell studied by the writer, a few hermetically sealed cells were made and briefly mentioned in another place.[1] The best of these cells, of the form known as the "sealed" cell, will be described somewhat in detail in the present paper. The advantage of a cell that can be completely closed by glass fusion is obvious. At the same time the cell should be moderately easy to construct, should embody strength and compactness, and should be perfectly portable. The present cell is primarily designed after the modified English Board of Trade model known as the B. O. T. "crystal" cell. It avoids the use of an amalgam button for negative electrode, which almost invariably ends by splitting the cell at the point where the negative terminal is fused through the glass, by the creeping of the amalgam along the wire. It includes the advantage of the "crystal" cell, *i. e.*, absence of diffusion lag, and has the extra advantage of being smaller and more sensitive. An important fact also is that the bulb of the cell containing the sensitive parts may be completely immersed in a water bath, or other form of thermostat, and at the same time is insulated thermally from the outside electrodes by the thick glass neck.

The arrangement of the cell will be best understood by reference to Fig. 1. In this the positive electrode is a platinum wire flattened at one end and amalgamated. The wire is enclosed in a glass capillary drawn from a small glass tube, shown in the figure, as the positive mercury cup. The negative electrode is a small zinc rod cast around a platinum wire and its enclosing glass capillary. This wire passes through the main fusion join *A*, and ends in the outer or negative mercury cup. As soon as the ingredients of the cell have been filled in through the open end, the final sealing of the cell is made by quickly fusing the glass in the narrow neck, *B*. By placing a small quantity of mercury in the cups, terminals, pilot leads, or other connections can be inserted for comparison or test. When the cell is to be sent away or carried any distance, the mercury can be at once removed from the cups. The contents of the cell being in a firm mass, the cell may be kept in any position.

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[1] H. L. Callendar and H. T. Barnes, Proc. Roy. Soc., 62, 117 (1897).

To construct the outer containing vessel, a glass tube, about one centimeter in diameter, is selected and cut about 15 or 16 centimeters long. This tube is drawn in the blowpipe flame at two places; one for the point of main fusion, *A*, Fig. 1, between 4 and 5 centimeters from one end, and the other *B*, so as to leave a bulb for the ingredients about 5 centimeters long. The restriction at *A* is to be left large enough to allow the zinc rod to pass through, while that at *B* should be large enough to allow of the insertion of the ingredients, through the remaining portion of the main tube not drawn down.

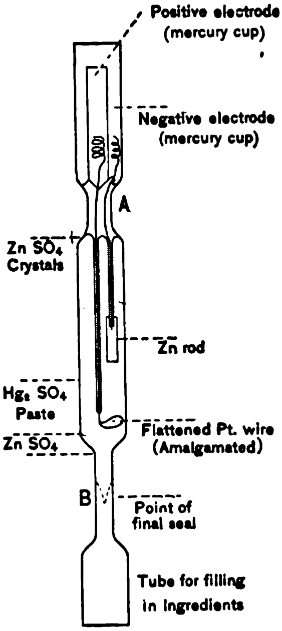
Positive electrode (mercury cup)
Negative electrode (mercury cup)
A
Zn SO₄ Crystals
Zn rod
Hg₂ SO₄ Paste
Zn SO₄
Flattened Pt. wire (Amalgamated)
B
Point of final seal
Tube for filling in ingredients

Fig. 1.

The positive electrode is made by drawing down a glass tube, of the same kind of glass as the main tube, 3 or 4 millimeters in diameter, into a firm strong capillary. This is shown at *P* in Fig. 2. The capillary should be long enough to reach from the main restriction, *A*, almost to the other restriction, *B*. A platinum wire, rolled flat for about a centimeter at one end, is passed through the capillary, and sufficient length left protruding into that portion of the tube not drawn down, which serves as the positive mercury cup. The capillary tube should be melted around the wire only at the end near the flattened portion. The amalgamation of the flattened end is done by heating red hot and cooling suddenly in pure mercury.

The negative electrode is made in the following way: a small mold of glass is constructed from a glass tube about 3 millimeters in diameter and is filled with particles of absolutely pure zinc. These zinc particles are melted, and while melted, a platinum wire and a portion of its enclosing glass capillary is thrust in. On cooling and solidifying the glass mold is broken away, leaving the small zinc rod firmly attached to the capillary tube. This terminal is shown at *N*, Fig. 2. It should be long enough to reach from the seal *A* to the middle of the cell. Before inserting in the cell, the zinc rod should be scraped clean and bright and amalgamated by dipping into pure mercury upon the top of which a thin layer of dilute sulphuric acid is placed. By this means the zinc surface comes in contact with the mercury immediately after cleaning in the sulphuric acid, which is essential to ensure perfect amalgamation. After this process the rod should be most carefully washed free from every trace of acid and dried with filter paper.

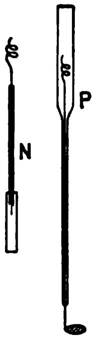

Fig. 2.

In making the main fusion, the two electrodes are placed in position by inserting them through the restriction at *A*, the positive electrode held central by means of small wedges, and the glass melted uniformly until it runs together around the two wires in a firm thick neck. The join is then carefully annealed to avoid cracking in the glass, which is liable to occur around the point where the positive mercury cup is sealed in, or at the point where the negative wire passes through. Before placing the electrodes in the cell, the ends of the wires for the mercury cups may be amalgamated.

In filling the cell, it is turned upside down and a small quantity at a time of moist zinc sulphate crystals pushed through the narrow neck by means of a small stiff glass rod. Once through the neck they can be shaken down into place. Crystals are added until the zinc rod is well covered. The paste, consisting of the usual mixture of pure washed mercurous sulphate and zinc sulphate crystals, is then pushed through in the same way. The paste should completely cover the flattened platinum wire. A few zinc sulphate crystals are then pushed through until they form a thin layer over the paste. The neck is then cleaned and dried as much as possible with filter paper, and fused together quickly in a small blowpipe flame. Very little heat is imparted to the glass beyond the neck, and what is passed to the ingredients is taken up by the thin layer of zinc sulphate crystals, thus avoiding any decomposition of the mercurous sulphate. The small bubble of air left after fusing the neck allows of the slight expansion of the materials and guards against the bursting of the cell. The completed cell, as in daily use in this laboratory, is shown by the photograph in Fig. 3, about two-thirds natural size.

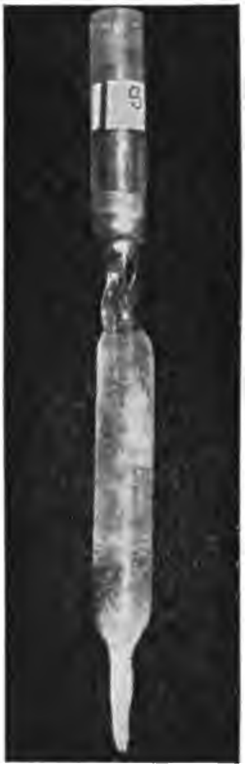

Fig. 3.

PREPARATION OF THE INGREDIENTS.

Mercury.—For amalgamation the mercury should be, when very impure, twice distilled in vacuum. When moderately pure, a single distillation is sufficient.

Zinc Sulphate.—The ease with which the very purest anhydrouszinc sulphate is obtained from any of the standard chemical manu facturers, makes it quite unnecessary to treat the solution with zinc oxide for neutralization, as recommended by the older processes. In order to obtain the heptahydrate crystals, the anhydrous salt is dissolved in water and a solution saturated at 30° C. prepared. While at 30° a small quantity of pure washed mercurous sulphate may be added, but it is not always necessary. It would depend on whether there was a trace of zinc oxide in the anhydrous salt. It, however, will do no harm and is immediately removed by filtering the solution in a water-jacketed funnel at 30°. The clear filtrate is cooled in melting ice, and the supernatant liquid poured off from the batch of crystals produced. Should a yellow turbidity appear in the process of cooling, it shows that the solution containing the mercurous sulphate has been heated to 35° or over. It is better to avoid the yellow turbidity altogether by not heating above 30°, but if present, the crystals may be remelted and the solution filtered until completely removed. The crystals produced in the melting ice are put aside in a stock bottle.

Mercurous Sulphate.—The purest mercurous sulphate should be obtained and washed by decantation several times, with pure distilled water. After the final washing the salt is caught on a filter and drained. When moderately dry it is transferred to a mortar and an equal quantity of zinc sulphate crystals added from the stock bottle. The mixture is ground into a thick paste with some pure metallic mercury and transferred to a second stock bottle, dark-colored or otherwise protected from the action of the light by black paper.

TESTS AND COMPARSIONS.

From a careful study of the change of E.M.F. with temperature, for the various types of cells devised, the following simple formula in millivolts was found to fit the observations most closely between 0°C. and 30°C.

$$E_t = E_{15} - 1.200\,(t - 15) - 0.0062\,(t - 15)^2. \cdots \qquad (A)$$

The analogous formula finally obtained by Kahle reads

$$E_t = E_{15} - 1.19\,(t - 15) - 0.007\,(t - 15)^2,$$

which is in remarkable agreement. By formula (*A*) the total change between 15 and 0°C. is 16.60 millivolts, while between 15° and 30°C it is 19.40 mv. At 30° the actual observed change diverges from this simple parabolic formula, increasing more rapidly as the temperature rises to 40°, when the sudden change of hydration of the zinc sulphate causes the E.M.F. to rise and follow a different curve. The character of the temperature curve above 30° and 40° has been studied by the author, and the details of the experiments have already been published.[1] In that place it was shown that formula *A*, when corrected by the additional term

$$- 0.00006\,(t - 15)^3 \cdots \qquad (B)$$

holds with great accuracy over the range 15° to 40°C. Above 40° the following expression represents the hexahydrate branch of the curve

$$E_t = E_{39} - 1.000\,(t - 39) - 0.007\,(t - 39)^2 \cdots \qquad (C)$$

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[1] H. T. Barnes, Journ. Phys. Chem., 4, 1 (1900).

It appears probable that the divergence from the simple parabolic formula (*A*) above 30° is due to some action of the mercurous sulphate at the higher points, which does not appear below 30°, and is also the cause, as has been pointed out, for the lower value given by the Clark cell for the temperature of inversion of the heptahydrate and hexahydrate (38°.78C.), as compared with the value given by solubility determinations, which places it more nearly at 40°C.

A sufficiently clear idea may be obtained of the accuracy and sensitiveness of the "sealed" cell by reference to the author's paper above referred to, on the inversion of the hepta- and hexahydrates of zinc sulphate in the Clark cell. However, various other data are available and hitherto unpublished on this form of cell, and may not be out of place here.

In Table I. are shown comparisons on several cells made at different times, with different materials. The differences in their E.M.F. are expressed in millivolts.

TABLE I.

Cell.	Jan. 1896.	Feb. 1896.	Feb. 1898.	May 1898.	Jan. 1899.
S_1	–0.11	–0.13	+0.07	+0.02	+0.00
S_3	–0.10	—	—	—	—
S_4	+0.05	+0.03	—	—	—
S_5	+0.10	+0.11	+0.13	+0.12	+0.17
S_6	+0.01	—	—	—	—
S_7	—	+0.01	—	—	—
S_{10}	—	—	–0.02	—	—
S_{11}	—	—	—	—	–0.05
nS_{11}	—	—	—	—	–0.05
S_{12}	—	—	—	+0.27	—
S_{13}	—	—	—	–0.05	—

The differences are of the order of $\frac{1}{10}$ of a millivolt; with the exception of S_{12}, which was tested rather too soon after being set up. Owing to its removal from the laboratory with S_{13} shortly after, no further tests were obtained. It is probable, however, that it may have contained a slight trace of acid from the amalgamation of the zinc, which could only be removed by very vigorous means, such as short-circuiting the cell, or otherwise by the lapse of time. In fact, one of the best methods of aging a cell and bringing it into a steady normal condition is by short-circuiting it with a copper wire for half an hour or so after setting up. The reads in Table I. show the variation from the mean of cells S_1 to S_6. Comparisons of cells S_{10} to S_{13} were made indirectly to cells S_1 to S_6 by comparison with five B.O.T. test-tube "crystal" cells that have been in the laboratory since 1895. A comparison of the means of the five sealed cells with the five crystal cells showed that the former exceeded the latter by only 0.08 millivolt. The five crystal cells have maintained their relative differences in a very satisfactory manner, as will be shown in another place.

During the progress of the author's experiments on the specific heat of water in terms of the international electrical units, two of these sealed cells were used and frequent comparisons made for a little over a year. These comparisons are given in Table II., and

Table II.

$S_6 - S_1$			$S_6 - S_1$		
Date of Comparison.	**Difference in mv.**	**Temp. of Comparison.**	**Date of Comparison.**	**Difference in mv.**	**Temp. of Comparison.**
Nov 16, 1897.	0.10	15°	Apr. 12, 1898.	0.08	15°
Feb. 4, 1898.	0.06	"	" 20, "	0.10	"
" 22, "	0.02	16°	May 4, "	0.12	"
Mar. 1, "	0.03	"	" 5, "	0.10	"
" 3, "	0.02	14°	" 9, "	0.13	"
" 5, "	0.06	15°	" 14, "	0.11	"
" 9, "	0.05	"	" 23, "	0.10	20°
" 10, "	0.07	"	" 28, "	0.15	15°
" 11, "	0.04	"	June 10, "	0.10	21°
" 14, "	0.07	"	" 13, "	0.19	18°
" 16, "	0.06	"	" 17, "	0.12	"
" 23, "	0.07	"	July 23, "	0.10	20°
" 25, "	0.07	"	Sept. 10, "	0.16	19°
" 31, "	0.09	"	Oct. 14, "	0.17	14°
Apr. 2, "	0.06	"	Dec. 14, "	0.03	15°

will serve to show the way in which these cells may be relied upon to maintain their relative differences.

The variation in the difference between the two cells is so regular and consistent, never exceeding in the extreme case much more than 1 in 10,000, that over extended periods the difference can be relied upon to one or two parts in 100,000. By taking sufficient care in the temperature and electrical measurements, this is about the order of agreement of this type of cell.

In regard to the temperature change and sensitiveness, some tests made on cell S_4 are given in Table III.

TABLE III.

Time after changing temp. of cell.	Total change of temperature.	Difference in mv. observed.	Diff. in mv. calculated by formula A.
5 minutes	15 to 0°C.	+15.90	+16.60
10 "	"	+16.44	"
60 "	"	+16.62	"
10 "	15 to 30°C.	−19.37	−19.40
30 "	"	−19.50	"

On returning to 30° from 50° C., the same cell showed a difference from its value at 15° of −19.62 mv. It is probable that had the cell been left at 30° in the first test longer than half an hour it would have shown a slightly larger difference. The total change between 15° and 30° by formula (*B*) is −19.60 mv., which is much closer to the observed value.

Table IV. contains tests made on cells S_4, S_5 and S_7, between 15°.36 and 23°.83 C.

TABLE IV.

Cell.	Total change in mv. observed.	Formula A.
S_4	−10.62	−10.62
S_5	−10.69	"
S_7	−10.62	"

Cell S_5 changed slightly more than the others, but the mean of the three cells will be a little in excess of formula (*A*), as is generally the case at the higher points.

The effect of a short circuit on one of these cells was shown by connecting the terminals of cell S_4 with a short, thick wire and obtaining comparisons of its E.M.F. during recovery after the wire was removed. In two and a-half minutes after short-circuiting for seven minutes, the E.M.F. was within three-tenths of a millivolt of its original value. In ten minutes it was within two-tenths, and it had completely recovered in thirty.

For an account of the thermostats, potentiometer and temperature measuring instruments used in these tests, reference should be made to the author's earlier papers (in loc. cit.), where also the method of carrying out the experiments is described.

Macdonald Physical Laboratory, McGill University, Feb. 14, 1900.

CONCERNING THERMAL CONDUCTIVITY IN IRON.

By Edwin H. Hall.

HAVING had occasion of late to examine with some care the information given by Landoldt and Börnstein and by Winkelmann concerning the thermal conductivity, *k*, of wrought iron and the temperature coefficient, *a*, of *k*, I have discovered so many inaccuracies, especially in Winkelmann, and have been so much impressed by the inconclusiveness of the showing, that I have felt moved to write a somewhat critical résumé of the methods and results in this limited but much worked field of investigation. I am the more induced to undertake this labor by the fact that even the most recent books published in English follow the custom of quoting the results obtained by Forbes as if they were the only ones worthy of especial mention, in spite of the fact that these results, for the best known of his bars, were long since disproved and superseded by work done on the same bar in the same laboratory.

It is true that Holborn and Wien published[1] some years ago the results of a discussion somewhat similar to that which I propose, but they did not publish their discussion; and, moreover, I find that in a number of particulars, especially in regard to the value of *a*, my conclusions are different from theirs. Where so much experimental work has been done, there is room for more than one attempt to estimate the value and results of that work. It is my hope to reach conclusions which may help to direct and make profitable future investigations. The recent interesting paper[2] of Liebenow, *Zur Thermodynamik der Thermo-ketten*, may seem to bring near the possibility of determining the thermal conductivity of metals from a study of their electrical conductivity and thermo-electric properties. But, whatever may be the final outcome, one of the first effects of Liebenow's paper must be to stimulate renewed interest in the results obtained from the methods heretofore used for the measurement of thermal conductivity.

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[1] Zeitschr. Ver. Deutsch. Ingen., Vol. 40, 1896.
[2] Wied. Annalen, June, 1899.

Landoldt and Börnstein give[1]

	Temp.	k.	Observer.
Eisen,		.1638	Neumann.
"	über 0°	.1587	Berget.
"	0°	.1988	Angström.
"	0°	.1665	Lorenz.
Schmiedeeisen,	0°	.2070	Forbes.
"	39°	.1485	H. Weber.

and the following temperature coefficients, that is, values of the constant a in the formula $k = k_0(1 + at)$:

Eisen		— 0.0002282	Lorenz.
" gewöhnl,	0 bis 300°	— 0.000611	Mitchell.
" gekühlt,	0 bis 300°	+ 0.000706	"

Winkelmann[2] gives in his table, without the dates,

Metal.	Temp.	k.	Observer.	Date.
Eisen,	0°	0.1665	Lorenz,	1881
"	0°	0.1988	Angström,	1861–4
"	über 0°	0.1587	Berget,	1890
"	ca. 15°	0.1648	Neumann,	1862
"	ca. 15°	0.1133	Wiedemann u. Franz,	1853
"	0°	0.1509	Mitchell,	1887
"	0°	0.172	Stewart,	1893
Schmiedeeisen,	0°	0.2070	Forbes,	1862–5
"	39°	0.1485	H. F. Weber,	1880

The temperature coefficients given by Winkelmann are

— 0.000038	from the work of Lorenz,
— 0.000517	" " " " Angström,
— 0.0006838	" " " " Forbes.

I shall follow in general a chronological order in my discussion, departing from it whenever there seems to be good reason for so doing.

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[1] Physikalische Chemische Tabellen.

[2] Handbuch der Physik., part 2 of vol. 2.

WIEDEMANN AND FRANZ.

"Es wurden zwei Stangen I. und II. von 5 mm. Dicke untersucht." This is, I believe, the whole explicit description of the iron, although it can be inferred from the description of the apparatus that the length of the bars was about 50 cm.

They worked with a considerable number of metals, but obtained comparative results only. It therefore seems idle to attempt now to derive absolute values of *k* from their data.

They appear to have been the first to make a systematic comparison of thermal and electric conductivities, although Forbes had many years before pointed out the apparent connection between these two properties in metals. Wiedemann and Franz did not measure, even comparatively, the electric conductivity of the bars; but trusted to the values found by others for similar materials. The table given by Winkelmann, Vol. 2^2, p. 277, to show their results differs in several particulars from that given by Wiedemann and Franz, and it is so badly aligned that in some cases it is impossible to tell to which metal the numbers given apply.

It is to be observed that Landoldt and Börnstein give no absolute values on the authority of these early experimenters.

Forbes, Angström and Neumann, working each by his own peculiar method, obtained results which they published about the same time. The method of Forbes was simpler, in theory, than the others, and will be discussed first.

FORBES (and MITCHELL).

No other experiments upon thermal conductivity are so familiar to English-speaking people as those of Forbes, and the method which he originated and used is so well known that description of its general features is unnecessary here; and yet it is time for us to recognize the fact that the numerical results obtained by Forbes have only an historical interest, and that, if their publication in physical tables of the present day is to perpetuate a belief in their numerical accuracy, it would be far better to exclude them altogether from such tables. This statement I shall undertake to verify by what follows.

Forbes used two wrought-iron bars of somewhat different sizes. The better known of the two, called D, he describes a "a beautiful bar, 1¼ inch square and fully eight feet long." He gives its specific gravity as 7.79. The other bar was 1 inch square. Reduced to the C. G. S. system, the values which he found for k from the two bars were

1¼ inch bar.	1 inch bar.	
0.207	0.1536	at 0° C.
0.1567	0.1293	" 100° "

The great difference between the values of k given by the two bars at the same temperature has been explained as probably due to a difference in the quality of the iron; but in view of the subsequent history of bar D, and the general average of values obtained for iron by other experimenters, this explanation seems inadmissible.

Each bar gave a large negative temperature coefficient for k. Tait, after Forbes, worked with bar D and applied corrections, which Forbes had not done, for change of specific heat with change of temperature. The result was to make the temperature coefficient almost or quite disappear. Then Mitchell worked with the same bar. In accordance with the advice of Tait he used sometimes the ordinary method of Forbes, heating one end of the bar while all the rest of its length was freely exposed to the air of the room, and sometimes combined the heating of one end with a cooling of the middle in order to get a steeper gradient of temperature in the cooler parts of the bar. This variation of method produced a marked change in the values found for k.

Forbes had expressed his results in terms of the foot, minute, and thermal capacity of a cubic foot of water. Mitchell departed from this tradition far enough to express his results in terms of the foot, minute, and thermal capacity of a cubic foot of the iron at some one temperature. It was therefore not necessary for him to determine the specific heat of the iron; but he did undertake to introduce into the values obtained for k at different temperatures correction for change of specific heat with temperature. It seems probable that he used a somewhat too large temperature coefficient in making this correction.

Certain thermometer corrections were applied by Mitchell, the effect of which corrections was to lessen considerably the values of thermal conductivity found for high temperatures as compared with those found for low temperatures.

The numbers which follow and the words accompanying them I have copied from Mitchell's paper:

Temperature C.		0°	100°	200°	300°
Iron (Ordinary).	Not corrected for specific heat change.	.0127	.0115	.0103	.0091
	Corrected for specific heat change.	.0131	.0123	.0115	.0107
Iron (Cooled Bar.)	Not corrected for specific heat change.	.01154	.01162	.01170	.01178
	Corrected for specific heat change.	.0119	.01274	.01358	.01442

"Iron (Ordinary)" refers to the "ordinary" Forbes's method, heat applied to one end of the bar the rest of which is exposed to the air. This is the "Eisen gewöhnl" of Landoldt and Börnstein's table. "Iron (Cooled Bar)" refers to the varied method, in which the middle of the same bar was cooled by a stream of water while the end was heated. This is the "Eisen gekühlt" of the same table. The great difference between the results of these two very similar methods throws much doubt upon the accuracy of the results obtained from either or both, so far, at least, as relates to the temperature coefficient, which has a large positive value in one case and a still larger negative value in the other case.

The values given by Mitchell are to be turned into C. G. S. units by multiplying them by 15.48 $C\rho$, where C is the specific heat at some one temperature, probably the temperature of the air, and ρ is the density. Winkelmann gets the value 0.1509 at 0°, which he puts opposite the name of Mitchell, by multiplying 0.0119 ("Cooled Bar," 0°) by $C = 0.105$ and $\rho = 7.8$. This reduction is probably not very far wrong, though a somewhat higher value for C might well have been used, but in reducing the values given by

Mitchell for *k* at higher temperatures, as Winkelmann does on page 280, he multiplies each by the specific heat of iron for the higher temperature in question, 100°, 200°, etc. The values which he thus gets for these higher temperatures are therefore doubly "corrected" for change of specific heat, once by Mitchell and again by Winkelmann. Fortunately, Winkelmann does not put these values into his final table.

Landoldt and Börnstein give no value of *k* from Mitchell; but, as we have seen, they give two values of *a*, its temperature-coefficient from his work, not perceiving, we may suppose, that "Eisen gewöhnl," "Eisen gekühlt," and "Schmiedeeisen" (Forbes) are one and the same bar *D*, one of the national monuments of Scotland. The temperature-coefficients which they give are obtained correctly from the values of thermal conductivity "corrected for specific heat change," quoted above from Mitchell. Taken together these two coefficients almost completely extinguish each other; and I can not help thinking that they should be allowed to do so entirely, and disappear from tables of physical constants.

Whatever may be thought of the accuracy of the value *k* at 0°; as finally obtained by Mitchell using his "cooled bar" method, it seems plain that the values 0.207 at 0° (Forbes) and 0.1509 at 0° (Mitchell) ought not to appear in the same table without an express statement that they were found from the same bar. Indeed the history of bar *D* goes far to show that the Forbes method is not well suited for the nicer problems of thermal conductivity, the uncertainty in estimating surface emissivity being too great.

We have not sufficiently definite data for a profitable estimation of the ratio between the thermal and electric conductivity in bar *D*.

STEWART.

R. W. Stewart also followed the method of Forbes, with some variations of detail, using a bar which he describes thus: "The iron bar was a ¾-inch square bar, about 4½ feet long, of ordinary commercial wrought iron, having its surface filed up and very lightly polished with black lead to secure uniformity of radiating power." "The density of the iron was found to be 7.556 at 0°

C." The specific heat was found by Bunsen's ice-calorimeter to be represented by the formula

$$C_t = 0.1095\ (1 + .00073t),$$

t referring to the centigrade scale.

The thermometers of Forbes were replaced by a single thermo-electric couple, made of "a good soft iron wire and German silver," each wire about 0.5 mm. in diameter. The depth of the holes bored in the bar to receive the thermo-electric junction was about 1.4 cm., the diameter about 0.1 cm.

"One end of the experimental bar was immersed in a vessel of melted lead maintained at a constant temperature, and the rest of its length was exposed to the air, but protected from draughts and external radiation by a wide trough of sheet zinc." The dimensions of this trough are not given, but from the figure shown it appears to have been about 27 cm. deep, the bar being suspended so as to hang about half way between the bottom and the top, which was not covered.

"In taking a series of observations of the distribution of temperature along the bar, the heating was allowed to go on for five or six hours, and then the deflections corresponding to each hole along the bar were observed, up and down the bar, time after time, until a set of perfectly constant and concordant readings were obtained."

"The distribution of temperature along the bars [copper as well as iron was studied] in the stationary state was determined by inserting one junction successively into the small holes drilled in the bar, while the other junction was maintained at a constant temperature[1] by inserting it in a hole in a short iron bar resting on the bottom of the zinc trough. The deflections obtained were, in this way, proportional to the difference of temperature between the several points on the bar and the temperature of the surrounding air. It was found necessary to carefully insulate the ends of the wires of the couple from one another just above the junctions, and from the bars in which they were inserted; this was done by filing down the wires, inserting a thin slip of silk between them, and then whipping them around with a fine silk thread. The outer surface of the thread charred slightly at the higher temperatures, but not sufficiently to destroy its efficiency." "The rates of cooling of the bars at different temperatures were determined by heating a piece of each bar, about a foot long, in a sand-bath to a temperature higher than required, and observing its rate of cooling under the same conditions as those to which the experimental bar was exposed. For this purpose it was suspended in the position originally occupied by the long bar, the thermo-electric junction inserted in a hole drilled at its middle point, and the deflection of the galvanometer observed at regular intervals during the cooling."

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[1] "This temperature was indicated by a thermometer placed with its bulb in a hole drilled in the [short] bar close to the small hole in which the junction was placed. In no case was the variation of temperature more than a fifth of a degree. The bar was a short massive iron bar, not the one used for cooling experiments."

Winkelmann quotes from the abstract of Stewart's work given in the *Proceedings* of the Royal Society. But in the *Transactions*, a mistake having been discovered, Stewart gives

$$k_t = 0.175\ (1 - .0015t).$$

I am of the opinion that little importance should be attached to the value of k obtained by Stewart and still less to his value of the temperature coefficient. A thermo-electric junction such as he describes, placed in a hole in the hot bar, but not in metallic contact with it, the greater part of the length of the wires being exposed to the air and the other junction being at a temperature near that of the air, would surely not have the temperature of the iron adjoining it. Moreover, we are told nothing about the location of the short bar carrying the colder junction except this, that it rested upon the bottom of the zinc trough—whether near the hot end or the cold end or the middle of the long bar, we do not know; but this one short bar was assumed to have the effective average temperature of the surroundings of the long bar and to hold the cooler junction at this same temperature.

Finally, the method of noting fall of temperature in the cooling bar, one foot long, by means of a single thermo-electric junction inserted at its middle point, is apparently inaccurate. The ends of the bar would certainly cool faster than the middle.

Near the end of his paper Stewart remarks, "Angstrōm's method should be adopted in preference to Forbes'." Having now done with researches by the method of Forbes, I return to the consideration of work done by his contemporaries.

ANGSTRÖM.

Angström used two bars of iron; the second, from which he obtained his best known results, was 117.8 cm. long and 3.5 cm. thick. I have found no description of its quality. Angström takes 0.8862 as the product of its specific heat by its density; but he uses precisely the same quantity for his other iron bar. It does not appear that he determined either the specific heat or the density.

Angström's general method of sending waves of temperature along the bar by alternate heating and cooling of one end, the remainder of the bar being exposed to the air, and noting the fluctuations of temperature at certain chosen points, is familiar to physicists and perhaps even more familiar to mathematicians. The mathematical discussion of the method, as given by Angström, treats specific heat and thermal conductivity as constants, it being assumed that the variation of these quantities, within the range of temperature fluctuations in the part of the bar studied, is too small to affect seriously the result arrived at. As to the validity of this assumption there has been discussion, which I do not propose to renew at present.

Whatever may be the merits of Angström's method, it is easy to show that he made near the end of his second paper[1] a large numerical error, the easy correction of which will bring his value of k for iron much nearer to the values obtained by most other investigators who have attempted to determine it.

The paper just referred to in Poggendorff contains the following record of data and experimental results:

EISENSTAB.

No.	Period.	$\frac{k}{c\delta}$	Temp.	
1	32′	10.487	43°.4	Einfache Reihe: Therm. No. 2 u 3
2	32	10.429	43 .7	Doppelte Reihe: Therm. *A* und *B*
3	32	10.279	18 .9	

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[1] Poggendorff, Vol. 118, p. 423, has a translation of the original paper.

This is followed, after a page or more devoted to the discussion of a similar record for copper, by a passage, quoted below, in which Angström professes to deduce a formula for k, as dependent on temperature, from the data which I have just given. A brief examination is enough to show that his formula, which has a negative temperature coefficient for k, cannot be derived from the numbers he refers to, which evidently give a positive temperature coefficient for k. Suspecting a mistake in the data as given in Poggendorff, I went to the original paper[1] and found the following table, which differs from that given above in one place of one number. We must therefore discard the data as given in Poggendorff, and consider whether the value given for k by Angström is consistent with the original data as here reproduced:

JERNSTÅNGEN.

No.	Period.	$\frac{k}{c\delta}$	Temp.	Anmärkningar.
1	32m	10.487	43°.4	Enkel serie ; therm. no. II. o. III.
2	32	10.429	43 .7	Dubbel serie ; therm. A o. B.
3	32	11.279	18 .9	" " "

The following discussion, by means of which Angström deduces his value of k, I quote from the article in Poggendorff, having compared the formulas in this discussion with those in the original passage and found them to be the same.

"Berechnet man den Werth von $\frac{k}{c\delta}$ aus den in der Tafel aufgestellten zwei letzen Bestimmungen für das *Eisen*, welche vollkommen vergleichbar mit einander sind, so erhält man

$$\frac{k}{c\delta} = 13.458\,(1 - 0.002874\,t)$$

und, wenn man setzt

$$c\delta = 0.8862$$

wird

$$k = 11.927(1 - 0.002874\,t)."$$

Now if we take the last two lines of the original data and calculate for them the value of $\frac{k}{c\delta}$ for 0°, we get 11.927. But this is precisely the value which Angström gets for k at 0°. According to the data referred to by Angström, he should get

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[1] Öfversigt af Kongl. Vetenskaps-Akademiens Förhandlingar, 1862, p. 24, Stockholm.

$$k_0 = 11.927 \times 0.8862 = 10.57;$$

and in general

$$k = 10.57\,(1 - .002874t),$$

for this revision requires no change in the temperature-coefficient which will be found perfectly consistent with the data.

The fact appears to be that Angström, having before him the quantity 0.8862 which he was presently to use as a multiplier, accidentally let it get in as a divisor, so that its final use as a multiplier merely gave back the value of $\frac{k}{c\delta}$, instead of giving the value of k.

In at least one other particular the calculations of Angström can profitably be revised. He treats the product $c\delta$ as a constant, using the same value for one bar at 0° as for another at 50°. But I shall now take the values of $\frac{k}{c\delta}$ which he gives as the results of experiments at 43°.7 and at 18°.9, apply to these the values of $c\delta$ which appear suitable to the temperatures, and work out thus values for k at 43°.7 and at 18°.9, and finally at 0°.

There can be no great error in taking 7.8 as the value of δ at all the temperatures considered, if Angström used good wrought iron, as in all probability he did. For the values of c I shall use numbers which are obtained by a combination of those given by Lorenz with those found by Naccari.

	Lorenz.				Naccari.		
Temp.	0°	50°	75°	100°	0°	15°	100°
Sp. ht.	0.1050	0.1107	0.1136	[0.1165]	[0.1080]	0.1091	0.1151

The numbers in brackets are my extrapolations. Then

$$\begin{array}{cc} 0^\circ & 100^\circ \\ c = \left\{\begin{matrix} 0.1050 \\ 0.1080 \end{matrix}\right\} 0.1065 & c = \left\{\begin{matrix} 0.1165 \\ 0.1151 \end{matrix}\right\} 0.1158. \end{array}$$

By interpolation

$$\begin{array}{cc} 18^\circ.9 & 43^\circ.7 \\ c = 0.1083 & c = 0.1106. \end{array}$$

Using these values of c, we get

$$k \text{ at } 43^\circ.7 = 10.429 \times 7.8 \times 0.1106 = 8.999,$$
$$\text{`` `` } 18^\circ.9 = 11.279 \times 7.8 \times 0.1083 = 9.528,$$

and by extrapolation k at $0^\circ = 9.93$. The general formula now becomes

$$k = 9.93(1 - .0021t),$$

in the units employed by Angström—the centimeter, gram and minute. Reducing to the ordinary system by dividing by 60, we have

$$k = 0.1655(1 - .0021t).$$

Holborn and Wien, in the paper to which reference has been made, give as the result of Angström's work

$$k = 0.1628\,(1 - 0.002874\,t),$$

but do not tell in what way they have revised his calculations.

The value 0.1988, k at 0°, which both Winkelmann and Landoldt and Börnstein give in their tables on the authority of Angström, is obtained from his 11.927 by dividing by 60. The temperature coefficient of k from Angström's work is given by Winkelmann as—0.000517. I do not understand how this value was found. A little earlier, on p. 282, Winkelmann gives Angström's result, revised for the change of $c\delta$ with temperature and reduced to C. G. S. units, as

$$k = 0.1842\,(1 - 0.001562t),$$

but here again there is nothing to show just how the numbers are derived. One value of the temperature coefficient is a little more than three times as great as the other.

Angström recognized the fact that for a satisfactory comparison of thermal and electric conductivities both quantities should be found for the same piece of metal in the same state. Accordingly, he attempted to measure the electrical resistance, x, of his bars, but encountered difficulties which baffled him, although he expressed the hope of overcoming them. Whether he finally succeeded I have not discovered. His results therefore tell us nothing directly in regard to the ratio $k \div x$.

Angström's method has not proved attractive to investigators in general. Tait tried it and gave it up. I am not aware that any-one except Angström has published results obtained by it for iron. Probably the very large value of k standing in the tables against the name of Angström has heretofore discredited his method. This consideration is now in great measure removed by the examination which has just been detailed. This objection, however, holds against all methods using temperature waves, that it is difficult for the ordinary reader, if not for every one, to see what effect will be produced in the result by small inaccuracies of theory or data. Such methods may give sufficiently accurate results, but they do not carry conviction.

NEUMANN.[1]

Neumann studied several metals. His whole published account of his work on these is contained in three pages; and the description is vague as well as brief. He used a method of variable temperature, heating one end of a bar or one section of a ring for a certain time and noting the changes of temperature after heating was stopped. Some features of his method can be seen in the work of H. Weber, which is presently to be described; for Weber avowedly followed Neumann as a guide, though perhaps with considerable deviations.

Neumann writes, "Les barres employées avaient 3 à 4 lignes de côté." The length is not given. "Dans quelques expériences j'ai aussi remplacé les barres par des anneaux." It seems not quite clear whether the results published were obtained from the bars or from the rings; but it is likely they came from the bars.

The specific gravity of the iron used was 7.74. Neumann gives 193 as the thermal conductivity of iron in the units employed, and says that this value can be reduced to the units used by Angström by multiplying by 0.0509. To reduce to C. G. S. units the

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[1] Ann. de Chemie et Physique, Vol. 66, 1862.

193 should be multiplied by 0.0509 ÷ 60, which gives $k = 0.1637$, practically the same value which Landoldt and Börnstein put opposite the name of Neumann. I find nothing in Neumann to indicate the temperature for which his value of k is supposed to hold.

Neumann was especially interested in the ratio between thermal and electric conductivity, and he deduced comparative values of this ratio for the several metals which he used. He did not, however, determine the electric conductivity in absolute measure, or give any data from which we can now reduce it to that measure; so that the real ratio between the thermal and electric conductivities of the iron which he used cannot be found.

H. WEBER.

H. Weber "of Brunswick," not H. F. Weber as in Winkelmann, used the method of Neumann. The particular volume, No. 146, of Poggendorff, in which the original account of his work appears, is missing from the set which is at my command, and I have therefore had recourse to the translation of his paper which appeared in the *Philosophical Magazine*, vol. 44, 1872.

1. "Iron rod (annealed)," round; density 7.761; specific heat 0.1125 (temperature not given); length of bar 23.035 cm., diameter 0.75168 cm. "The density and the specific heat were specially determined, the latter according to the method proposed by Neumann."

After describing briefly the method of Angström, Weber says, "Neumann has now shown that the same problem can be treated in another way, and one more favorable for observation—and that it is more suitable for the determination of the two conducting-powers ["internal" and "external"] to subject not merely *one* end of a bar regarded as unlimited, but *both* ends of a bar of infinite length to the same periodic change of temperature in such a manner that when the two ends in the 0th period [really the *first* period of observations after the bar has been brought into a state of regular alternation] have the temperatures u_0 and u_1 they in the next following take the temperatures u_1 and u_0, in the 2d period (on the contrary) they are in the same state as in the 0th, and in the 3d as in the 1st, etc." The temperature u_1 was that of steam, u_0 about 4°.6. The length of a period was 5 minutes.

Assuming for the purposes of mathematical discussion, that the specific heat, the density, the internal and external conductivities, are constant quantities, an assumption which Weber defends only as an unfortunate necessity, he proceeds to show that determinations of k, the internal conductivity, and h, the external conductivity, can be found from observations of the temperature of the air, the temperature of the middle point of the bar (which temperature should prove to be a constant), and the difference of temperature between two other properly chosen fixed points on the bar at various intervals during each period.

Thermoelectric effects, by means of wires soldered to the bar at the points mentioned, were used to measure the temperatures required. "A thin iron wire and a thin German-silver wire were soldered in the middle of it [the iron rod] * * * opposite to each other. The ends of these two wires were soldered to two copper wires," the points of junction being near together "in the surrounding air" at a point the temperature of which was read from a sensitive thermometer. At two other points, one of which was $\frac{1}{6}$ of the length of the bar from its end and the other $\frac{4}{6}$ of the length from the same end, "two German-silver wires were soldered to the iron bar * * * and their ends, at about 6 inches from the bar, were soldered to the two copper wires which led to the same galvanometer as the wires before mentioned." Readings of the deflections produced by this latter thermo-electric couple were taken at intervals of 15 seconds, and, as this deflection was changing rapidly during much of the time, a careful study of the characteristics of the galvanometer was necessary. The mean temperature of the air was about 5°.7; that of the middle point of the bar, 33°.1. The estimated mean temperature of the whole bar during a "period," of 5 minutes, was 39°.23, which, according to Weber, "can only be regarded as a rough approximation."

There is a tone of intelligence, frankness and thoroughness about the paper of Weber which inspires confidence. I cannot help feeling, however, that the description and discussion of the thermo-electric devices is too meagre. How thick and how long were the "thin" wires soldered to the middle of the bar? How much solder was used in making the junctions, and how great was the thickness of solder between the wires and the bar? These seem to me important questions in a case where, as here, some of the points of attachment were rapidly varying in temperature and all were much above the temperature of the room. It is unfortunate that Weber did not make such variations in his thermo-electric apparatus as to remove reasonable doubts concerning its accuracy.

On the other hand we feel somewhat reassured by the fact that he did vary considerably the surface condition of the bar, without finding any corresponding important change in the value derived for the thermal conductivity.

After giving a series of values obtained from "even periods," which agree well among themselves, and a corresponding self-accordant series from "odd periods," he says, "If we take the mean of the values of K [the internal conductivity] and H [the external conductivity] given by the even periods, and likewise of those derived from the odd periods, we obtain:

$$K = 15.14, \qquad H = 0.00266,$$
$$K = 14.56, \qquad H = 0.00266."$$

The difference between "odd" and "even" is attributed to "want of absolute coincidence of the soldering-places with the points $x = \frac{1}{6}l$ and $x = \frac{4}{6}l$." For the mean we have

$$K = 14.85 \qquad H = 0.00266.$$

"The surface of the iron bar had not the highest polish. In order to try whether the quality of the surface exerted a perceptible influence on the value of K, the previous experiments were repeated with the same iron rod after its surface had been evenly coated with soot from a gas flame. The thickness of the coating was such that the metallic surface was just perceptible through it. Here the duration of the periods amounted to 10 minutes, in order to make the observations in even and odd periods alternately, not consecutively as before." The mean results from the iron thus coated were

$$K = 14.79 \qquad H = 0.00328,$$

an extremely satisfactory agreement for K, with the previous result.

It is unfortunate that the temperature at which the specific heat was found is not given. The value used, 0.1125, seems rather high for the mean temperature of the bar. An error of two or three per cent. may occur here, making the value obtained for K too large by that amount.

Weber's values of K and H are expressed in terms of millimeters, milligrammes and seconds. To reduce to C. G. S. units we must divide K by 100 and H by 10. We are concerned with K only, from which we get

$$k = 0.1485,$$

as given by Landoldt and Börnstein and by Winkelmann. This is referred to the mean temperature of the rod, about 39°.

Weber found the ratio of the thermal conductivity to the electric conductivity, which henceforth I shall call the ratio $k \div x$, to be 2458×10^4 at 44°.3C, in the units which he used. Reduced to the C. G. S. system this ratio becomes 2458.

L. Lorenz.[1]

The iron used by Lorenz appears to have been exceptionally pure. It had a high density, 7.828 at 0°, and an electric conductivity greater than Landoldt and Börnstein quote for any other specimen of iron.

Lorenz gives from his own observations on this iron

Sp. Ht.	**Elect. Cond.**
$C_0 = 0.1050$	$x_0 = 10.374 \times 10^{-5}$
$C_{50} = 0.1107$	$x_{100} = 6.628 \times 10^{-5}$
$C_{75} = 0.1136$	

His bar was 30 cm. long and 1.5 cm. in diameter.

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[1] Wied., N. F., 13, pp. 422 and 582, 1881.

In his main experiments Lorenz used two methods. One was that of Forbes with various improvements. The other, which he considers the more exact, consisted in measuring the time rate of increase of mean temperature throughout a certain part of a bar, with a known difference of temperature gradient at the ends of the portion considered, and with a surrounding jacket kept at a known fixed temperature by flow of cold water or steam. Moderate differences of temperature between different parts of the bar and between the bar and jacket were used. The temperature measurements were made by means of thermo-electric junctions, usually of copper and German-silver, but sometimes of copper and iron. Heat was brought into the bar by end contact with a bar of copper kept hot by means of a lamp. Of course it was necessary to know the density of the bar and its specific heat at various temperatures. Careful observations to determine these quantities were made upon the bar itself or upon a portion cut from it.

It is impossible to read the papers of Lorenz on thermal conductivity without being impressed by the thoroughness of his work and the probable accuracy of his results. But his method as a whole does not strike one as simple or easy to carry out, and there is one detail of the apparatus which suggests to me the possibility of considerable error. The thermo-electric junction commonly used consisted of a copper wire 0.01 cm. thick and a German-silver wire 0.03 cm. in diameter soldered together end to end in a straight line. This junction rested within a hole 0.04 cm. diameter bored straight through the bar from side to side, and both wires were insulated from contact with the metal of the bar, the German silver within the hole being covered with a thin wrapping of silk for this purpose. For the purpose of finding the temperature gradient leading heat into or out of the parts considered, "thermo-electric double elements were used which consisted of short German-silver wires with both ends soldered to copper wires." Now it seems probable that the ends of any one of these German-silver wires would, under the circumstances described, differ decidedly less in temperature than the parts of the bar at the two holes connected by the wire. This objection must have occurred to Lorenz, though he does not, so far as I have observed, mention it. It is easy to see that error from this defect of apparatus would in some measure eliminate itself, since it is the difference of two temperature gradients which enters into the calculations. Moreover, Lorenz states that he sometimes used copper-iron thermo-electric junctions, and sometimes junctions made by copper wires joined directly to the bar, without finding any material difference in the results obtained. Finally, it is to be said that the Forbes method, which Lorenz used for checking the results obtained by his first method, gave nearly the same values for thermal conductivity in most cases. The agreement in the case of iron was especially good, the Forbes method giving a result about 1½ per cent. smaller than the other. It is true that similar errors may affect both methods as applied by Lorenz.

The original method used by Lorenz was particularly well designed for giving the temperature coefficient of k; and the value which he found for this coefficient would be entitled to much consideration, even if it should appear that the values which he found for k were somewhat erroneous.

Lorenz found, by his preferred method, for iron

$$k_0 = 0.1665, \qquad k_{100} = 0.1627,$$

in C. G. S. units.

The temperature coefficient from these values is 0.0002282.

Landoldt and Börnstein report k and its temperature correctly from Lorenz; but Winkelmann in deducing the temperature coefficient forgets to divide by k_0.

The ratio $k \div x$, as found by Lorenz for his iron bar at 0°C., is 1605.

BERGET.[1]

Berget made first a determination of the thermal conductivity of mercury in absolute measure, and afterward found the thermal conductivity of various solid metals, including iron, by comparison with the mercury. He found the gradient of temperature in a vertical column of mercury, of which the upper end was exposed to steam and the lower end was kept cool by melting ice. A "guard ring" of mercury, exposed to the same terminal temperatures as this column, surrounded the column and prevented lateral escape of heat. The amount of heat transmitted by the shielded column was calculated from the amount of ice melted at its lower end, this latter quantity being found sometimes by use of a Bunsen ice-calorimeter, sometimes by weight. The temperature gradient was measured by means of thermo-electric couples, formed by iron wires leading through the guard cylinder into the interior column, with which they were in metallic contact.

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[1] Compt. Rend., 1887–'90. Journal de Physique, Vol. 7, p. 2, 1888, and Vol. 9, p. 135, 1890.

The self-consistency of the results which Berget obtained for mercury is marvelous, but not greater than the admirable simplicity and directness of his method seems capable of giving. There was no need of determining specific heat or "surface conductivity."

In working with solid metals Berget superposed a column of mercury upon a column of the solid, each surrounded by its "guard ring," found the gradient of temperature in each by thermo-electric means after a stationary condition was reached, and then deduced the conductivity of the solid from that of mercury by the simplest of calculations.

The whole account of his work given by Berget is brief, and some details of apparatus or procedure are described less fully than the reader could desire; but the general method is so excellent that one can have little doubt concerning the accuracy of all parts of the investigation. It is therefore the more to be regretted that Berget has not told us the degree of purity, or even the specific gravity, of the metals which he studied. Perhaps it is not yet too late for this omission to be made good.

He determined, for solid metals, the mean thermal conductivity between 0°C. and 30°C. and declared its value to be, for iron, 0.1587.

He found the mean electrical conductivity of the same bar of iron to be, between 0°C. and 30°C., 9.41×10^{-5}.

The ratio of the mean values of the two conductivities between these limits of temperature is therefore, according to Berget,

$$k \div x = 1687.$$

This may be taken as equivalent to

$$k_{15} \div x_{15} = 1687.$$

BEGLINGER.[1]

Probably the broadest piece of work ever done by one person upon the thermal conductivity of iron is that of W. Beglinger, who determined the density, specific heat and thermal conductivity of 52 pieces of iron, including steel and cast iron, of many varieties. The following passages freely translated from his own descriptions will give an account of the general method and arrangement of apparatus.

"A broad cylinder, which in all points had the same initial temperature *N*, was from a determined moment, which we take as the zero point of the time, subjected to a continuous cooling by keeping its base continually at the temperature 0°, while the remainder of its surface was allowed to give off heat into a space kept likewise at the constant temperature 0°. The time-rate of fall of temperature at any point of the cylinder can be arrived at by calculation ; and after certain simplifications of the expression thus obtained for the temperature, there results the method for the determination of the internal heat conductivity." The method thus indicated is credited to H. F. Weber,[2] who had applied a somewhat similar method to the study of heat conduction in several metals[3] about 1880. "The temperature was measured thermo-electrically. Since it would not do to break the continuity of the material by means of small canals, one junction of the thermo-element was placed at the middle of the upper end surface [of the cylinder], the other junction kept at the constant temperature 0° [so-called] of the cooling water. The thermo-electric current was led through a galvanometer of such a character as to permit the variable electromotive force to be followed exactly after the lapse of a few seconds." * * * "From a given moment, after the cylinders had been heated, its lower end surface was played on by a stream of water 3 cm. thick from the public water supply (having a pressure about 7 atmospheres), while care was taken that the upper end as well as the curved surface of the cylinders should not be touched by the water."

"In the center of the upper end surface [of the cylinder] a drill hole 0.2–0.25 cm. broad and 0.15 cm. deep was filled with a piece of soft copper. In this was bored a fine hole in which the junction [of the thermo-element, of thin iron and German-silver wire] could be easily and securely placed."

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[1] Verhandlungen des Vereins zur Beförderung des Gewerbfleisses, vol. 45, 1896, pp. 33–61.

[2] Berliner Monatsberichte, 1880, and Wied. Ann., Bd. 13.

[3] Vierteljahrsschrift der Naturforschenden Gesellschaft in Zurich, vol. 25, 1880.

The narrowest cylinders used by Beglinger had a diameter of nearly 6 cm.; the greater number had a diameter of 15.8 cm. or more. The least length was 4.44 cm.; the length of the greater number ranged from 6.5 cm. to 8 cm.

Each cylinder when in use was mounted on, or rather in, a broad thick supporting plate of iron, having at its center a hole widening conically toward the upper face and just wide enough at the bottom face to admit the cylinder. The lower end surface of the cylinder was made flush with the lower surface of the plate, being supported in that position by three small lugs attached to the under face of the plate. There could be side contact between cylinder and plate only along a strip ½ mm. high. The narrow space between the two was filled with a cement consisting of yellow wax and "Kolophonium." Above the cylinder was placed on the plate a double-walled cap of sheet copper containing water of the same temperature as the other cooling water. The water jet, about 3 cm. wide, was directed against the middle of the base of the cylinder from a distance of 3 or 4 cm. with very great force, and "erst am Rande des Gestelles [support] in seiner Bewegung in horizontaler Richtung gehemmt, nahm es seinen Weg nach unten." At the maximum this stream delivered about 3.7 liters of water per second with a velocity of 5.3 meters per second. A considerable reduction of the flow was accompanied by a perceptible loss of effectiveness of the stream.

The warming of the cylinder was effected by means of a Bunsen burner; and it lasted from 3 to 5 minutes, at times as much as 10 minutes. After the operation the under surface of the cylinder was always carefully cleaned and the water jet was put into position. Two or three minutes were thus allowed to elapse, in order that for the subsequent cooling it might be assumed that the initial temperature was everywhere the same. The warming was observed by means of the galvanometer, and "im Allgemeinen wurde nicht über 40° Temperaturdifferenz hinausgegangen." I think 40° is here a mistake for 4°; for I find later, as a typical case, "Kühlwassertemperatur 3°.3 C." * * * "$\bar{u} = 2°.0$; also gilt das Resultat für die Temperatur 5°.3 C."

As the "external" conductivity plays a certain part, though a subordinate one, in the cooling, certain direct observations upon rate of cooling by external conductivity alone were made.

In some cases the effect of removing the thermoelectric junction from the middle of the cylinder end to an excentric position was tried.

Each cylinder was tested in both of its upright positions; that is, the face which had been the base in one set of observations was made the top in another set; this gave a rough test of the homogeneity of each cylinder, which test all of the wrought-iron cylinders except one and all of the steel cylinders except one bore well. But with 14 out of 31 cast-iron cylinders the values of k obtained from the two positions differed from each other by 2% or more.

For the determination of its specific heat each cylinder was heated in a water bath to "30, 40 oder 50°," from which it was after a time quickly transferred to the calorimeter. The giving out of its heat in this calorimeter occupied from 4 to 8 minutes.

Beglinger uses the minute as his unit of time. His values of k are, therefore, to be divided by 60 in order to reduce them to the C.G.S. system, and the reduction is made in the following table, which gives the results of his work with malleable iron (Schmiede-eisen):

Schweisseisen.					
No.	l	$2R$	ρ	c	k
	cm.	cm.			
1. Schweisseisen, Gerlafingen	7.387	15.91	7.835	0.1110	0.1502
2. Sandwyk-Eisen	6.992	19.96	7.884	0.1104	0.1492
3. Siegener Schweisseisen	6.987	19.96	7.803	0.1107	0.1329
4. Schweisseisen, Gerlafingen (E.)	6.982	19.96	7.812	0.1115	0.1279
5. Frischfeuereisen, Gerlafingen	7.447	20.05	7.693	0.1112	0.1263
6. Packeteisen "	7.285	20.05	7.806	0.1110	0.1262
7. Hayange, No. 2, gewalzt	6.970	15.92	7.521	0.1134	0.1078
8. Hayange, No. 2, geschmiedet	6.987	15.91	7.550	0.1122	0.1112
Flusseisen.					
9. Flusseisen, Gerlafingen	7.275	20.05	7.849	0.1108	0.1346
10. Flusseisen.	6.045	15.87	7.657	0.1125	0.1241

l = length.
$2R$ = diameter.
ρ = density.
c = specific heat.
k = thermal conductivity.

Beglinger does not tell, as a rule, for what temperature his values of c or of k were determined; but from various remarks it seems probable that the temperatures for k lie between 5°C. and 20°C. The mean temperatures for which c was experimentally determined probably lay ten or fifteen degrees higher. Whether the values of c, as given above, are reduced to the lower temperatures for which k holds, we are not, I believe, directly told; but it appears as if these values of c were used in finding k. The mean of the values given for c is about what Naccari and also Lorenz would give for the specific heat of iron at 50° C.

Without questioning the great value and merit of Beglinger's work, one may suspect his results for k to be affected by considerable errors. For example, the description of the method of warming the cylinder leaves it very doubtful whether its temperature was everywhere the same at the beginning of the cooling. Moreover, it is even possible to doubt whether the lower face of the cylinder, in spite of the great size and velocity of the water stream directed against it, had everywhere the temperature of the water in the stream. The parts directly attacked by the full force of the stream would, perhaps, soon attain and keep a temperature very nearly the same as that of the stream itself; but other parts would probably be less affected. We might, therefore, expect narrow cylinders to be cooled somewhat more rapidly, other things being equal, than broad cylinders. As to the possible action along the curved surface of the cylinder Beglinger remarks: "Es ist zu bemerken, dass aus der Erwärmung des Gestelles [during the warming of the cylinder by means of a Bunsen burner] kein Fehler entstehen konnte; ebensowenig ist ein Einfluss der Kittschicht, trotz vielfacher Modifikationen konstatirt worden." It is to be remarked, however, that if there is any flow out through the cement layer during the cooling, this flow would hasten the cooling more, other things being equal, in narrow cylinders than in broad cylinders. We find here, then, two possible influences tending to make k appear greater in narrow cylinders than in broad cylinders. Reasoning in this way, I was curious to see whether the results, as given by Beglinger, would give any indication of such a result. It happens that he used 11 pairs of cast-iron cylinders, the individuals of each pair being of the same material and differing, so far as was known beforehand, only in the diameter, which was about 6 cm. for one and 16 cm. for the other. Taking mean values I find

	ρ	c	k
For broad cylinders,	7.102	0.1152	0.1192
For narrow cylinders,	7.093	0.1159	0.1225

The mean k found for the narrow cylinders is, therefore, about 2.5% greater than the mean k found for the broad cylinders; and as the mean density and mean specific heat are nearly the same for one set as for the other, it seems probable that the apparent difference in the mean values of k is due to some fault in the method of experimentation.

This is not a very grave criticism of the work of Beglinger, which from its scope was necessarily somewhat rough in details; but it should be borne in mind hereafter, if there should be a disposition on the part of any one to discredit the values obtained by others on the ground that these values do not agree with any of those given by Beglinger. The maximum value of k which he found for wrought iron is 0.1502, which is a very little smaller than the value which I have found for a less dense piece of iron, about 5% less than the value found by Berget, and 10% less than the value found by Lorenz for iron of very nearly the same density, as Beglinger's iron.

Beglinger's results seem to show that, in the case of malleable iron, density is an important, though by no means the sole, factor in determining the value of k. The greatest value of k was not found in the most dense piece, No. 2, nor the least value of k in the least dense piece, No. 7; and yet, when the values of k for the ten pieces of malleable iron are plotted as ordinates with the densities as abscissas, it appears that an increase of 1% in the density is accompanied by an increase of several per cent. in the value of k, and there is an indication that the rate of increase of k rises with the increase of ρ. The other variables upon which k in soft iron depends are, according to Beglinger, very obscure. He calls attention to the fact that k in cylinder No. 1, which came directly from the iron-works, was about 15% greater than k in No. 4, a cylinder of the same material which came through a commercial firm. The density and specific heat were about the same in one of these cylinders as in the other. It seems a pity that serious effort was not made, by dissection and minute examination of these two cylinders, to discover the cause of their great difference in k.

It may be well to add a few words concerning the results of Beglinger for steel and cast-iron.

The 11 cylinders of the steel group were all "Flussstahlsorten." In these

ρ varied from 7.84 to 7.92,
c " " 0.1110 to 0.1135,
k " " 0.1327 to 0.0940.

There was among these no apparent connection between ρ and k.

Among the 11 cylinders four pieces are numbered twice each, once in the soft condition and once in the hard condition. "In 3 Fallen hatte der gehärtete Cylinder ein kleineres k als der weiche, aber nur um 11, 5 und 6% ; in eniem Falle hatte sogar der gehärtete Cylinder ein um 2% grösseres Leitungsvermögen." "Die Ergebnisse für das Härten bestätigen die Resultate des Herrn Kohlrausch nicht." In fact, Beglinger comes to the conclusion that hardness and softness in steel, whether due to its mode of manufacture or to subsequent treatment, has comparatively little influence on k. It is to be observed, however, that Beglinger used pieces of steel about 4.5 cm. thick, and, as he does not describe his method of hardening them, it may well be doubted whether he succeeded in making them very hard throughout.

Within the cast-iron group of 31 cylinders,

ρ varied from 6.849 to 7.217,
c " " 0.1120 to 0.1210,
k " " 0.0932 to 0.1533.

Here, as in steel, there is no apparent connection between ρ and k. It seems to be a matter of mere chance whether a very dense piece of cast iron has a high, low or medium thermal conductivity. In the eleven pairs of cast-iron cylinders already mentioned, the individuals of each pair being ostensibly of the same material, Beglinger found always a difference of *k* between these individuals, the smallest difference being less than ¼%, the largest 21%.

Beglinger gives the chemical analysis of none of his cylinders, and expresses a doubt as to whether such analysis would clear up the differences observed, in view of the comparatively small effect produced by the presence of a large amount of carbon. "Der gebundene Kohlenstoff drückt beim Gusseisen das *k* etwas hinunter im Beispiel des Cylinders No. 40 gegenüber No. 33, wo der Kohlenstoff zum grössten Theil in Form von Graphitblättchen ausgeschieden ist." "Ueber den Einfluss von Mangan, Silicium und Schwefel etc., auf das Wärmeleitungsvermögen von Stahl und Eisen fehlt jeder Anhaltspunkt."

I find from the values given by Beglinger:

for 10 pieces of soft iron the mean $k = 0.1300$,
" 11 " " steel " " " $= 0.1100$,
" 31 " " cast iron " " " $= 0.1175$.

HALL.

The general method which I have followed in measuring the thermal conductivity of a plate of wrought iron is the same as that used by Mr. Ayres and myself in our work on the thermal conductivity of cast iron.[1] Certain changes of apparatus or procedure, the most important being the substitution of platinum thermometers for thermoelectric junctions in measuring the change of temperature of the water passing over the disk, will be described in the Proceedings of the American Academy; but the character of the iron used and the results obtained will be given here.

The metal was Yorkshire wrought iron, recommended to me by an engineer friend of much experience as the softest wrought iron to be found in the Boston market. At the place of purchase the metal was described to me as containing 99.7% iron and no carbon. Chemical analysis made at my instance showed

iron	99.93%,
carbon	0.059%.

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[1] Proceedings of American Academy of Arts and Sciences, Vol. 34, p. 283, 1899.

The values which I find for the thermal conductivity, k, of this iron are, at

28°.2C.	0.1528,
58°.3C.	0.1514,

if the specific heat of water at each of these temperatures is called 1. This would give for the temperature coefficient 0.0003, very nearly.

If the values of k are revised in accordance with the values proposed by Winkelmann[1] for the specific heat of water at the given temperatures, they become at

28°.2C.	0.1513,
58°.3C.	0.1511,

and the temperature coefficient deduced therefrom will be too small to be worth writing down, 0.0000?, let us say.

But the recent work of Callendar and Barnes[2] gives for the specific heat of water

0.9992	at 25°C.
0.9987	at 30°C.
0.9992	at 55°C.
1.0000	at 60°C.

If these values given by Callendar and Barnes are adopted as correct, my first values of k will stand almost without change; and I shall therefore leave them for the present without correction for variation in the specific heat of water.

My value for the temperature coefficient, 0.0003 or a little less, is, so far as it goes, a corroboration of the substantial accuracy of the temperature coefficient found by Lorenz, 0.0002282, although it may be doubted whether the last three figures of this number are of much significance. This agreement is eminently satisfactory to myself; for a comparison of the work of Lorenz with that of other investigators has convinced me that his value of the temperature coefficient is entitled to an especial degree of confidence.

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[1] Part 2 of Vol. 2, p. 340.

[2] This REVIEW, April, 1900.

Measurements of the electrical resistance of the iron were made on 9 cylinders, each 2 cm. long and about 0.23 cm. in diameter, cut from the same great bar as the disk on which the measurements of k were made. The length of the cylinders, like the thickness of the disk, was taken parallel to the length of the bar; and the cylinders were cut from a part of the bar adjacent to that from which the disk was cut. The extreme difference in the specific resistances of these cylinders was apparently about 5 per cent. The mean specific resistance was found to be 12240 at 18° C. The mean specific conductivity, x, at the same temperature would, therefore, be 817×10^{-7} C. G. S.

SUMMARY AND DISCUSSION.

In accordance with the preceding exposition I have made the following table of results for wrought iron, in which $\rho =$ the density, $x =$ the electric conductivity, $k =$ the thermal conductivity :

Observer.	ρ	$x \times 10^7$	k
Method of steady flow with "external conduction."			
Forbes,	7.79	?	0.207 (?) at 0°C. (see Mitchell).
Mitchell,	7.79	?	0.1509 (?) at 0°C. (sp. ht. uncertain).
Stewart,	7.556	?	0.175 (?) at 0°C.
Methods using waves or oscillations of temperature.			
Angström,	?	?	0.1655 (?) at 0°C. (sp. ht. uncertain).
Neumann,	7.74	?	0.1637(?) at ? (account too brief).
H. Weber,	7.761	604 at 44°C.	0.1485 at 39°C.
Methods of rise or fall of temperature with internal and external conduction.			
Lorenz,	7.828	1037.4 at 0°C.	0.1665 at 0°C.
Beglinger,	7.74	?	0.130 (?) at ? (method rough).
Method of steady flow with guard ring or jacket.			
Berget,	?	941 at 15°C.	0.1587 at 15°C.
Hall,	7.785	817 at 18°C.	0.1528 at 28°C.

Temperature coefficient of k.

Lorenz, $a = -0.0002282$.
Hall, $a = -0.0003$ or slightly less.

The only two values here given for a were obtained by very different methods, which, however, have this very important feature in common, that each of them used precisely the same piece of metal in finding the low temperature value of k as in finding its high temperature value, the whole apparatus being used at two different levels of temperature, with little or no relative change within it in passing from one level to the other. In the Forbes method of finding a, followed by Mitchell and Stewart, the high temperature value of k is found at one part of a bar and the low temperature value at a different part, where the quality of the metal may not be quite the same. For this reason, as well as for others which will be apparent to any one reading the preceding discussion of work by the method of Forbes, I have retained none of the values of a obtained by that method. The value to be derived from the data given by Angström seems also too uncertain for a place in this summary.

If we take -0.00025[1] as the value of a in wrought iron of high grade, I believe that we may have much confidence in the ciphers and some confidence in the first digit.

In four cases we have both k and x in C. G. S. units. Reducing to 0°C., using -0.00025 as the value of a and $x_0 = x \div (1 - 0.0051t + 0.000013t^2)$ as the temperature formula for electrical conductivity, we get in these four cases

Observer.	ρ	x	k	$k \div x$
Lorenz,	7.828	10374×10^{-6}	0.1665	1605
Berget,	?	10160 "	0.1593	1568
Hall,	7.785	8958 "	0.1539	1716
H. Weber,	7.772	7540	0.1500	1989

The mean of the four values here given for k is 0.1574. The mean value deduced by Holborn and Wien was 0.156 at (?)°C.

It is plain that diminution of density is here accompanied by fall of both x and k. It will be remembered that Beglinger's results also, for soft iron, indicated a fall of several per cent. in k with a fall of one per cent. in ρ. It would be unwise, however, to draw definite numerical conclusions from the numbers before us; for all the values of x and k are in some degree uncertain, and in the case of Weber's iron even the ρ is a little dubious; for he does not tell the temperature for which ρ is 7.761, and I have assumed that it was 39°C., which he gives as the mean temperature for which he determined k. We should be in better position for speculations if we knew the value of ρ in the iron used by Berget. The x and k for this iron seem to require a high density.

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[1] Holborn and Wien, *loc. cit.*, retaining the values of a given by Mitchell and Stewart, get, as the most probable value, $a = -0.001011$.

Although k appears to increase rapidly with increase of ρ, x increases still more rapidly, so that, with one exception, we find $k \div x$ to decrease with increase of k. This exception may be due to entirely obscure causes lying in the difference of quality of the iron used by Lorenz and Berget respectively, or it may be due to errors of measurement by one or both of these investigators. To determine this ratio within 1 % for any piece of iron would be a considerable achievement. It is worth while to inquire what values of this ratio were obtained by Lorenz and by Berget for the other metals which were examined by both of them. Brass being omitted as a too uncertain quantity, these metals were copper, tin, lead and antimony. Berget's results being given for 15°C., we must reduce them to 0°C.; and for this purpose can not do better than to take the temperature coefficients found by Lorenz. Using these we get for the ratio $k \div x$ at 0°C.

	Lorenz.	Berget.
Copper,	1574	1516
Tin,	1635	1727
Lead,	1627	1532
Antimony,	2011	1629

Thus we have, including iron, four cases out of five in which the value of $k \div x$ found by Berget was less than that found by Lorenz. I am, therefore, inclined to attribute the exception above noted, in the column $k \div x$ for iron, to errors of measurement, and to hold it as a rule that in iron the ratio $k \div x$ decreases with increase of k.

In conclusion I have to make the following suggestions :

1. That every one who undertakes to determine the thermal conductivity of any metal should determine also its electrical conductivity and thermoelectric quality, and should give in general the most definite attainable account of its physical and chemical properties.

2. That every one should have regard not merely to the probable accuracy of his method but also to its convincingness, lest his re-

sults, however accurate, may be received with doubt and so prove ineffective.

3. That the general methods used by Berget are peculiarly simple, direct and convincing; and that his probably very accurate determination of k for mercury puts at the disposal of all other investigators a standard substance by comparison with which they may, using his guard-ring method, measure the conductivity of other metals with far less labor than if they undertook independent absolute determinations.

A striking example of the uncritical use of old data relating to thermal conductivity is furnished by the paper of Liebenow referred to at the beginning of this article. Liebenow derives from his theory the following formula for the thermo-electromotive force in any metal:

$$\frac{dE}{dT} = \pm 2.04 \sqrt{\frac{RL}{T}},$$

where dE is the electromotive force in volts,

dT " difference of temperature in degrees C.,
R " specific resistance in ohms,
L " thermal conductivity,
T " absolute temperature.

He then says, " For copper one finds accordingly at 15° C., if one takes 0.8 as the specific thermal conductivity of copper,

$$\frac{dE}{dT} = \pm 2.04 \sqrt{0.8 \times 0.000001635 \div 288} = \pm 0.0001375 \text{ volts.''}$$

Why Liebenow took the value 0.8 he does not say. The values given by Winkelmann on the authority of various observers range from 0.6296 to 1.11. But having taken this value for copper he follows the curious course of calculating the thermal conductivity for iron by multiplying 0.8 by the value which Wiedemann and Franz found for the thermal conductivity of iron as compared with copper, as if there had been no improvement on the work of these observers during the past forty-five years. The electric conductivities are taken from Matthiessen's values. Accordingly Liebenow gets for iron

$$\frac{dE}{dT} = \pm\ 128.2 \text{ microvolts},$$

"and one gets for the iron-copper element — 9.3 microvolts [that is, 128.2 — 137.5]; which agrees very well with observations."

To show how much importance Liebenow attaches to the case of iron and copper, I will quote one more passage. "Since the electric stream in the copper-iron element, under 276°, flows from copper to iron at the warm junction, though the electromotive force of the copper is in this case larger than that of the iron, it follows that the direction of these forces is opposite to that of the heat flow; that is, the positive electricity is in these metals, with unequal temperature, driven toward the warmer places. Moreover, since all metals investigated by Matthiessen give, in combination with copper, smaller electromotive forces than 137.5 microvolts, one must conclude that all metals when unequally heated become electrically *positive* at the warm end."

But let us see how the argument would run if we were to use the results obtained by Lorenz and by Berget, both excellent and, as compared with Wiedemann and Franz, recent experimenters.

From the data given by Lorenz we get for copper at 0° C.

$$\frac{dE}{dT} = 2.04\sqrt{\frac{RL}{T}} = 2.04\sqrt{\frac{k}{xT}10^{-9}} = 0.0001549 \text{ volt},$$

and for iron at the same temperature,

$$\frac{dE}{dT} = 2.04\sqrt{\frac{k}{xT}10^{-9}} = 0.0001564 \text{ volt}.$$

Thus we find $\frac{dE}{dT}$ greater for iron, the opposite of what Liebenow finds. At 15° C., the temperature used by Liebenow, the excess of $\frac{dE}{dT}$ for iron would be a little greater than at 0° C.

From Berget's data we get at 15° C. for copper

$$\frac{dE}{dT} = 2.04\sqrt{\frac{1.045}{651300 \times 288}} = 0.0001519 \text{ volt},$$

and for iron at the same temperature,

$$\frac{dE}{dT} = 2.04\sqrt{\frac{0.1587}{94100 \times 288}} = 0.0001561 \text{ volt.}$$

Here again, $\frac{dE}{dT}$ is greater for iron than for copper.

However sound Liebenow's general theory may be, it seems plain that his computations should be revised.

CAMBRIDGE, May 12, 1900.

SPARK-LENGTH OF AN ELECTRIC INFLUENCE MACHINE, AS MODIFIED BY A SMALL SPARK FROM THE NEGATIVE SIDE.

By W. J. Humphreys.

ABOUT ten years ago I noticed that the discharging distance between the poles of an ordinary Toepler machine could be very greatly increased by the aid of small sparks, taken by the hand or otherwise, from the negative pole, or from any part of the machine in metallic contact with it; and also that this property was peculiar to the negative side, no such effect being produced by sparks taken from any of the positive parts.

Naturally I supposed that a description of this phenomenon, so easily and in fact often accidentally obtained from so common a machine, was to be found in the journals and larger treatises, but I have not been able to find any reference to it. I therefore venture to publish a short account of my experiments along this line, trusting that the results, if not new and interesting to all, may at least be so to many.

For the sake of convenience and clearness I refer to the accompanying sketch, in which J, J, are the two small leyden jars usually on electrical influence machines, W the wire connecting their outer coatings, A and B the discharge balls. C and D connect, one with the positive the other with the negative comb of the machine.

Let the machine be run continuously, and let the distance between A and B be gradually increased till the sparks cease to pass. If a small spark is now drawn from any part of the machine metallically connected with the *negative* pole, the dilectric between A and B will in general break down and a spark of full magnitude pass between them.

In this way the sparking distance may be increased anywhere up to seventy-five per cent. or even more, the extent of increase depending on many things as will appear from the experiments described below.

The experiments were usually made with the machine in constant action, but this was only a matter of convenience as the discharges between the poles could still be forced, by the aid of a small spark from the negative side, after the machine had come to rest, but of course before the potential had dropped very greatly.

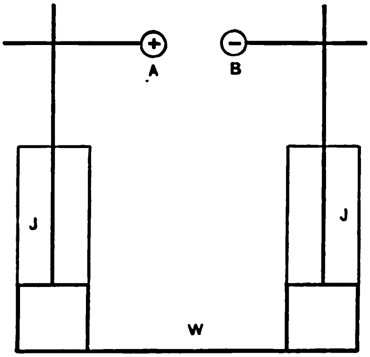

Fig. 1.

The phenomenon has been tried, and with uniform success, with several machines; among them an old type Holtz, Toepler and Wimshurst machines. Most of my observations however have been made with a two-plate Wimshurst machine, the plates being twenty inches in diameter, of ebonite and furnished with metallic sectors.

When the poles were equal in polish and otherwise it was, as one would expect, a matter of indifference which was positive and which negative, the forced spark being obtained equally well in each case.

The phenomenon has been tested under quite a number of conditions—some of which are the following. Where no statement to the contrary is made, the condensers belonging to the machine were used, and both poles kept polished.

1. Roughened negative pole. Results never good; the negative pole lost its charge so rapidly in the form of a brush that it was impossible to obtain sparks of any considerable length.

2. Polished negative pole. Results generally very good.

3. Condensers of very small capacity. Increase in spark gap slight.

4. Condensers of large capacity. Spark gap not much increased.

5. No condensers. Spark gap not affected by spark from either pole.

6. Condensers belonging to machine, and polished negative pole. Results generally excellent.

7. State of the atmosphere such as to prevent high potential, machine giving only short sparks. Results less pronounced, the poorer the working of the machine.

8. Condition of the atmosphere such as to cause unusually long sparks. Increase of spark-length ordinarily not very great.

9. Working of machine such as to give sparks of medium length, from eight to twelve centimeters with the Wimshurst machine. Increase of spark-length most pronounced, often nearly doubling the distance between the poles.

10. Poles separated very widely. In this case a spark could not be made to pass the whole distance, but each time a small spark was taken from the negative side a discharge passed from the positive pole and lost itself along many branches into the air. Nothing of this kind happened at the negative pole when a similar spark was drawn from the positive side.

11. Discharge poles removed some distance, about seventy centimeters, from the revolving disks. Results the same as when the poles were in their usual positions.

12. Outsides of condensers connected by metal rod. Results excellent.

13. Outsides of condensers connected by secondary of a large induction coil. Increase in sparking distance still very pronounced, but volume of spark diminished.

14. Outside of condensers connected by a wet string. Increase in sparking distance still decided, but volume of spark greatly diminished.

15. Condensers completely insulated from each other. Outsides temporarily grounded or not, but finally left insulated. In this case there was not the slightest indication of a spark on taking small discharges from either pole.

Various other arrangements were tried, but probably these are the most important, and from them it may be gathered that if the poles of an electrical influence machine be separated beyond their sparking distance, they will still allow a spark to pass, either as a unit from pole to pole, or as one losing itself in branches, each time a small spark is drawn from the negative side of the machine, provided that the outer coatings of the condensers are connected by a conductor; and further that nothing of the kind takes place when the condensers are insulated from each other, nor when the small spark is taken from the positive side of the machine.

It may be well to state that the size of the small spark necessary to cause the discharge between the poles is not the same under all conditions. When the poles are separated but little beyond their sparking distance it may be very slight, but has to be much larger when the distance between the poles is greatly increased.

The experiments of Lodge and others on the importance of electrical surges and oscillations in the production of sparks, would incline one to attribute the above phenomenon to some such cause. There is, however, one important objection to this explanation. If the phenomenon is really due to surges or oscillations, it is not clear why they should not be equally great, and the results just as pronounced when the side spark is taken from one pole as when it is taken from the other. Still, the fact that the phenomenon ceases when and only when the outer coatings of the jars are so insulated that there can be no oscillations or even surges between them, well nigh compells the acceptance of the above explanation, but in a distinctly modified form.

Let the jars be fully charged, and the poles separated too far to allow sparks to pass under ordinary conditions. The field of force due to these charges is a complicated one, depending of course to some extent on neighboring objects, but as a rough approximation it may be regarded as made up of lines of force between the poles A and B say of the figure, and a much greater number of lines of force from inner to outer coatings through the walls of the jars *J*, *J*. If the jars are insulated from each other a spark drawn from either *A* or *B* will decrease the strain between them. I assume the sparks to be taken with objects in connection with the earth, that is at zero potential. If, on the other hand, the outside coatings of the jars are so connected that oscillations or even one-way surges may pass between them a spark from either pole will cause an impulsive rush of the opposite charge to the other pole and a consequent rise of its potential. That the break-down takes place only when the auxiliary spark is taken from the negative side indicates that the dielectric is far more sensitive to shocks next the anode than it is next the cathode.

There are other indications, at least one of which I shall discuss in another paper, of the superior sensitiveness to electrical disturbances of the region about the positive over that about the negative pole. But this idea of sensitiveness of the dielectric next the anode, when under usual conditions, is already supported by certain experiments of Faraday, Macfarlane and De la Rue and Hugo Müller. In speaking of Faraday's experiments on discharges between spheres of different sizes, J. J. Thomson[1] says: "We may express this result by saying that when the electric field is not uniform the gas does not break down so easily when the greatest electromotive intensity is at the cathode as it does when it is at the anode."

To explain this sensitiveness at the anode, we may assume that the molecules of the dielectric between the poles are polarized and formed into Grotthus' chains, and further that these chains are more easily broken down or ionized by shocks at one end than by shocks at the other or by steady strains. This hypothesis offers a common explanation of the above phenomenon and Jaumann's discovery[2] that a gas is electrically weaker under oscillating fields than under steady ones.

In the various experiments of Jaumann and other workers on the effects of oscillations in producing sparks there are no indications, so far as I know, whether it is the positive to negative pole or the reverse impulse that produces the spark, but if the above assumptions are true it would seem to be the former and not the latter.

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[1] Experimental Researches, p. 170.

[2] Sitzb. d. Wien Akad., 97, p. 765, 1888.

Doubtless more or less close mechanical analogies to the assumption of a Grotthus' chain more sensitive to shocks at one end than at the other could be found, but at present I shall not search for them, since the assumption is offered more as a means of coördinating this with other experiments, and of suggesting possible lines of further work than as a complete and final explanation.

Rouss Physical Laboratory,
University of Va., March, 1900.

INDEX TO VOLUME X.

A.

Acetylene flame, temperature of the, *Nichols*, 234.

Acoustic analyses of vowels from the phonographic record, *Bevier*, 193.

Adams, C. F., An apparatus for determining the coefficient of expansion of air at constant pressure, 178.

Aimé, G., *De l'influence de la pression sur les actions chemiques*, 190.

Air at constant pressure, apparatus for determining the coefficient of expansion of, *Adams*, 178.

Ångström, K., An objective representation of the hysteresis of iron and steel, 74.

Anodes, size of as influencing deposit of silver voltameters, *Merrill*, 167.

Arc, a dissociation theory of the electric, *Child*, 151.

B.

Baker, W. C., A lecture demonstration in wave motion, 175.

Barnes, H. T., An hermetically sealed type of Clark standard cell, 268.
  Variation of specific heat of water between 0° and 100° C.

Barnett, S. J., The influence of electrification on the surface tension of water and mercury, 65.

Barus, Carl, Scheiner's *Strahlung und Temperatur der Sonne*, 126.
  Transmission of the ionized exhalations of phosphorus through air and other media, 257.

Benzene, revision of some thermal data concerning, *Tsuruta*, 116.

Bevier, L., Jr., Acoustic analysis of vowels from the phonographic record, 193.

Book reviews.
  American telephone practice, *Miller*, 192.
  Elements of alternating currents, *Franklin and Williamson*, 189.
  Elements of physics, *Crew*, 254.
  Geschichte der physikalischen experimentierkunst, *Gerland und Traumüller*, 127.
  Influence de la pression sur les actions chemiques, *Aimé*, 190.
  Lehrbuch der experimental Physik, *Wüllner*, 63.
  Ostwald's Klassiker der exakten Wissenschaften, Nos. 99 and 100, 63.
  Strahlung und Temperatur der Sonne, *Scheiner*, 126.
  Text-book of physics, *Watson*, 255.
  Text-book of physics, *Wentworth and Hill*, 187.
  Traité élémentaire de mechanique chemique fondeé sur la Thermodynamique, *Duhem*, 191.

Boyle's law apparatus, an efficient, *Humphreys*, 123.

Boynton, Gibbs' thermodynamic model, 228.

Cajori, F., Gerland and Traumüller's Geschichte der Physikalischen Experimentierkunst, 127.

Callender, H. L., Variation of specific heat of water between 0° and 100° C., 202.

Carpenter, H. V., A new method of comparing two self-inductances, 52.

Cell, an hermetically sealed type of Clark standard, Barnes, 268.

Child, C. D., A dissociation theory of the electric arc, 151.

Copper voltameter, polarization and internal resistance of, *Moore*, 34.

Crew, H., *Elements of Physics*, 254.

D.

Density and elasticity of the ether, *Fessenden*, 1, 83.

Dissociation theory of the electric arc, *Child*, 151.

Duhem, P., *Traité élémentaire de mechanique chemique fondeé sur la Thermodynamique, Duhem*, 191.

E.

Elasticity and density of the ether, *Fessenden*, 1, 83.

Elasticity of rods, effect of magnetization on, *Stevens*, 161.

Electric and magnetic quantities, determination of nature of, *Fessenden*, 1, 83.

Electrification, influence of on the surface tension of water and mercury, *Merritt and Barnett*, 65.

Ether, elasticity and density of the, *Fessenden*, 1, 83.

Expansion of air at constant pressure, apparatus for determining the coefficient of, *Adams*, 178.

F.

Fessenden, R. A., A determination of the nature of the electric and magnetic quantities, and of the density and elasticity of the ether, 1, 83.

Franklin, W. S., Aimé's *De l'influence de la pression sur les action chemiques*, 190.

  Duhem's Traité élémentaire de Mechanique chemique fondeé sur la Thermodynamique, 191.

  Elements of Alternating Currents, 189.

G.

Gerland, E., *Geschichte der Physikalischen Experimentierkunst*, 127.

H.

Hall, E. H., Concerning thermal conductivity of iron, 277.

Hill, G. A., *Text-book of Physics*, 187.

Hodges, N. D. C., Law of distribution of velocities among gas molecules, 253.

Humphreys, W. J., An efficient Boyle's-law apparatus, 123.

  Spark-length of an electric influence machine as modified by a small spark from the negative side, 301.

Hysteresis of iron and steel, an objective representation of, *Ångström*, 74.

I.

Inductance, self, a new method of comparing, *Carpenter*, 52.

Internal resistance and polarization of the copper voltameter, *Moore*, 34.

Iron and steel, an objective representation of the hysteresis of, *Angström*, 74.

Iron, concerning thermal conductivity of, *Hall*, 276.

Iron group, magnetic molecular susceptibility of salts of the iron group, *Liebknecht and Wills*, 215.

Iron, soft, on the change in length in an alternating magnetic field, *Austin*, 180.

K.

Kimball, A. L., Crew's *Elements of Physics*, 254.

L.

Liebknecht, O., magnetic molecular susceptibility of salts of the iron group, 215.

Living body, metabolism of matter in, *Rosa*, 129.

M.

Magnetic and electric quantities, determination of nature of, *Fessenden*, 1, 83.

Magnetic field, on the change in length of soft iron in, *Austin*, 180.

Magnetic molecular susceptibility of salts of the iron group. *Liebknecht and Wills*, 215.

Magnetization, effect of, on the elasticity of rods, *Stevens*, 161.

Mercury and water, influence of electrification on the surface tension of, *Merritt and Barnett*, 65.

Merrill, J. F., The influence of temperature, pressure, used solutions, and sizes of anodes on the deposit of, 167.

Merritt, Ernest, The influence of electrification on the surface tension of water and mercury, 65.

Metabolism of matter in the living body, *Rosa*, 129.

Miller, K. B., *American Telephone Practice*, 192.

Moore, B. E., Polarization and internal resistance of the copper voltameter, 34.

N.

Nichols, E. L., Miller's *American Telephone Practice*, 192.
  On the temperature of the acetylene flame, 234.
  Ostwald's *Klassiker der Exakten Wissenschaften*, 63.
  Watson's *Text-book of Physics*, 255.
  Wüllner's *Lehrbuch der experimental Physik*, 63.

P.

Phosphorus, transmission of ionized exhalations of, through air and other media, *Barus*, 257.

Polarization and internal resistance of the copper voltameter, *Moore*, 34.

Pressure, influence of on the deposit of silver voltameters, *Merrill*, 167.

R.

Rosa, E. B., On the metabolism of matter in the living body, 129.

S.

Scheiner, *Strahlung und Temperatur de Sonne*, 126.

Self-inductances, a new method of comparing two, *Carpenter*, 52.

Silver voltameters, the influence of temperature, pressure, used solutions, and size of anodes on the deposit of, *Merrill*, 167.

Smith, H. B., Franklin and Williamson's *Elements of Alternating Currents*, 189.

Solutions, used, influence of on the deposit of silver voltameters, *Merrill*, 167.

Spark-length of an electric influence machine as modified by a small spark from the negative side, *Humphreys*, 301.

Specific heat of water between 0° and 100°C., the variation of, *Callender and Barnes*, 202.

Standard cell, Clark, on hermetically sealed type of, *Barnes*, 268.

Steel and iron, an objective representation of the hysteresis of, *Ångström*, 74.

Stevens, J. S., Effect of magnetism on the elasticity of rods, 161.

Surface tension of water and mercury, influence of electrification on, *Merritt and Barnett*, 65.

T.

Temperature, influence of, on the deposit of silver voltameters, *Merrill*, 167.

Temperature of the acetylene flame, *Nichols*, 234.

Thermal conductivity in iron, *Hall*, 277.

Thermal data concerning benzene, revision of, *Tsuruta*, 116.

Thermodynamic model, Gibbs', *Boynton*, 228.

Thompson, J. O., Wentworth and Hill's *Text-book of Physics*, 187.

Traumüller, F., *Geschichte der Physikalischen Experimentierkunst*, 127.

Tsuruta, K., Thermodynamic notes (No. 10); Revision of some thermal data concerning benzene, 116.

V.

Velocities, law of distribution of, among gas molecules, *Hodges*, 253.

Voltameter, copper, polarization and internal resistance of, *Moore*, 34.

Voltameters, silver, the influence of temperature, pressure, used solutions, and size of anodes on the deposit of, *Merrill*, 167.

Vowels, acoustic analysis of, from the phonographic record, *Bevier*, 193.

W.

Water and mercury, influence of electrification on surface tension of, *Merrill and Barnett*, 65.

Water, variation of specific heat of, between 0° and 100° C., *Callender and Barnes*, 202.

Wave motion, a lecture demonstration of, Baker, 175.

Watson, W., *Text-book of Physics*, 255.

Wentworth, G. A., *Text-book of Physics*, 187.

Williamson, R. B., *Elements of Alternating Curreuts*, 189.

Wills, A. P., Magnetic molecular susceptibility of salts of the iron group, 215.

Wüllner, A., *Lehrbuch der Experimental Physik*, 63.

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